

How Not to Exploit North Sea Petroleum

THE Public Accounts Committee in the House of Commons has done a public service by making the most thorough examination so far of the way in which the British government has awarded licences to petroleum companies for the exploitation of petroleum and natural gas in the North Sea. The committee's report (*First Report from the Committee of Public Accounts 1972-73*, HMSO, £1.55) has a sorry tale to tell. First, it complains that the government, which might have been forgiven in 1964 for charging too little for exploitation licences at a time when the potential of the North Sea was more or less unknown, has failed to stiffen its terms as it has become clear that the North Sea is a potentially valuable oil field. It is especially disconcerting that the Department of Trade and Industry, now the responsible authority, should have failed to revise its policy after an experiment in tendering for blocks of territory in the North Sea attracted bids totalling £37 million from the petroleum companies. The committee has also drawn attention to the way in which most of the petroleum companies operating in the North Sea may be able to offset their tax bills by subtracting from them taxes already paid elsewhere in the world or losses and development costs incurred in other kinds of operations, running tanker fleets, for example. Although the British government has in the past few years been subjected to some ill-informed criticism on the grounds that it had been too lenient with the petroleum licensees, it is hard to see how it will be able to shrug off the cogent criticisms of the Public Accounts Committee.

The emergence of the North Sea as an important source of energy has been rapid and full of surprises. In the early sixties, it was thought likely that the North Sea would yield substantial amounts of natural gas, for much the same geological reasons as the Netherlands was able to find gas beneath land now dry or close offshore in the early nineteen fifties, but the discovery of substantial oil fields has been a big surprise. From the start, however, the British government has taken the view that the reserves, unknown though they may have been a decade ago, should be exploited as rapidly as possible. This was the starting point for the original decision that the companies licensed to exploit a block of territory should make annual payments which will rise from £10,000 a year in the seventh year of exploitation to a maximum of £72,500 a year as time goes on and that they should pay a royalty of 12½ per cent of the value of the petroleum products extracted. These terms appear to have been influenced by two separate considerations. First, the British government seems to have been anxious not to make life too difficult for the oil companies operating in the Middle East and North Africa by setting terms which might have given the OPEC countries an excuse for increasing their demands. Second, the government was persuaded by its officials that it would in any case recover something like a total of half the value of the oil extracted from the North Sea by taxation of company profits, an assumption that

would have been correct if only the government had understood its own taxation system as well as the oil companies appear to do. The prospect now, however, is that the yield to the British government from the exploitation of the North Sea reserves will be much less than it might have been, and certainly less than other countries such as Norway now have good reason to expect. The government has only itself to blame if it is now subjected to a chorus of demands from the Labour Party that North Sea gas and oil should be nationalized.

At this stage, it is hard to know what can be done by less drastic policies. The Public Accounts Committee says that the government should consider a revision of the method of taxation on petroleum products, and there would be no inequity in a levy of a fixed amount on each barrel of oil recovered from the North Sea. The government's difficulty, however, is that its agreements with the oil companies do not give it automatic access to operating costs in the North Sea, with the result that it cannot, without the cooperation of the companies, fix a levy that will not be inequitable to the companies and which will nevertheless ensure that the market price of North Sea oil is comparable with the world market price at any time. That is another defect in the present arrangements, which have given the oil companies leases on the most productive parts of the North Sea for a total of 46 years.

On Tuesday this week, the Chancellor of the Exchequer acknowledged that the Public Accounts Committee is right, and agreed that the tax rules should be changed, but there are more general issues to be decided. In the first place, the British government should recognize that the North Sea is not the last of the offshore areas in which oil is likely to be found. Perhaps the Public Accounts Committee is unnecessarily pessimistic in supposing that, now that it has helped to shut the stable door, the horse has already bolted. What is needed is some administrative device for making sure that the selling price of oil and gas should be roughly comparable with the world price for these materials and that it should rise as the world price rises, and yet also allow exploiting companies nevertheless to make a decent profit. There is a certain entrepreneurial simplicity in the device, much favoured in the United States, for putting blocks of land up for auction, and certainly that is a method which does not require that civil servants should understand the economics of the petroleum industry. It has much to commend it. But it would be still better if there were government machinery for fixing the price of oil in the North Sea from time to

Editor of *Nature*

Formal applications are invited (see page xi of this issue) for the post of Editor of *Nature*, which becomes vacant later this year.



time in tune with the world market price, and if the difference between proven production cost and selling price were shared equitably between the government and the companies. This does not imply nationalization but it does amount to close economic control.

The second general question to which the government should now pay some attention is the untested hypothesis which appears to have guided its exploitation policy for the past decade—the notion that the exploitation of the North Sea should be as rapid as possible. The error implicit in this assumption is that the need to save foreign exchange, and to insulate the British economy from the rising price of OPEC oil, is paramount. The truth is, of course, that no amount of success in the North Sea will make it possible for British needs for liquid fuels to be met exclusively from local sources. Moreover, there would clearly be great hazards in the long-term if the British economy were able to withdraw from the world market and chose to do so. What would happen then when the North Sea reserves are played out, half a century or so from now? How would British industry re-enter the world market? And would it be welcome there? The truth is that the country will remain dependent on a variety of sources of gas and petroleum and that it cannot hope to diminish the economic pressures of the international market by a more rapid exploitation of the offshore reserves. It would be much better, in the circumstances, if the speed of exploitation were determined not by the rate at which physical arrangements can be made for extracting gas and petroleum but by a prudent economic assessment of the most sensible rate of exploitation.

University Computers

THE Computer Board for Universities and Research Councils is a worthy and almost typically British institution. It owes its existence to the recognition, in the 1960s, that universities would not be able to equip themselves with modern computing facilities out of the recurrent grants from the University Grants Committee and that, in any case, structural innovations would be needed if quite small universities were to be given access to quite large machines. The result is that there are now a number of regional centres to which most universities have convenient access, while the growth of computer power has increased substantially. In its latest report, the Computer Board says that in March 1972, the effective machine capacity was ten times that available to the universities in 1965, and the Computer Board now promises that by 1974, the capacity accessible to universities will have doubled again. This is a creditable performance, even if the pace of installation in the past few years has fallen below the estimates of demand prepared at the outset of the Computer Board's existence. And even if it has been necessary to meet some of the demands by throwing the IBM 370 machine at the Rutherford High Energy Laboratory into the common pool, it is also fair to say that this programme has been carried through at comparatively little cost. In the last complete financial year, the Computer Board spent only £9.3 million on its activities.

It remains an important question to know whether the Computer Board should continue indefinitely to exist as a kind of fosterchild of the University Grants Committee

and the Science Research Council. At the beginning, the frustration of university researchers seeking access to computer time was a powerful demonstration of the need for university access to large modern computers, and the pattern of regional centres which has grown up is a sensible way of meeting the need. But now that the pattern has been established, and now that there is at least a chance that adequate capacity will be available in the near future, is it not time to consider whether the Computer Board should not be subsumed within either the Science Research Council or the University Grants Committee and preferably the latter?

The advantages of a merger are not clear-cut but they should be considered seriously. First of all, there is now, or may be in the next few years, a tendency to assume that because the Computer Board exists, the universities themselves and the University Grants Committee as a whole should wash their hands of computer policy. The truth is, however, that computers are now as much a part of the essential equipment of a university as were lecture halls and laboratories in the 1940s. It would be in everybody's long-term interest if the provision of computer facilities was regarded as an inescapable charge on the collective budget of the universities. Then the existence of regional computer centres, which have outlived the jealousies which attended their creation, would be a useful organizing principle to include within the British university system. Whether the universities like it or not, financial pressures in the years ahead are certain to force on the University Grants Committee proposals for the grouping and common use of other expensive facilities. It would help the committee, in these battles yet to come, to be able to show that it could run a sensible organization for the sharing of computer time. But the time has also come when universities could play a more vigorous part in specifying the kinds of computer facilities they consider to be necessary. For one thing, the provision of computer facilities for instructional purposes has lagged behind the need, but the universities themselves are the best agents for developing the equipment needed for that purpose.

The question of whose computers should be bought for the universities is a continuing conundrum. The introduction to the Computer Board's report makes it clear that the publication of the 1971 report was abandoned because of differences between the Computer Board and the government on the policy of helping British manufacturers. So far as can be told, these differences have been patched up but not resolved. And in the past two years, the Conservative government has become as attached as its Labour predecessor was to a policy of helping British computer manufacturers to sell their products even in the face of the apathy of potential customers. But is it really sensible that the government should pursue a policy which denies to British universities the best machines for the sake of keeping IBM at bay when the universities themselves are now the most fruitful source of the men and women who will, in the years ahead, be able to help British manufacturers improve their products? The trouble is, of course, that the British government's policy on the computer industry is still inadequately defined. There is very little chance that British computer manufacturers will be able to develop internationally competitive machines in the near future, and there is no reasonable hope that their task will be simplified if they are given an unfair share of the domestic

market in the next few years. What the government has to decide is what kind of computer industry it wants to have ten years from now and how much it is prepared to pay in development costs to achieve that target. It is unthinkable, in present circumstances, that British companies could succeed independently of their competitors in Europe, and even if it did, it would find itself hamstrung by the procurement policy of European governments. The moral is that the British government should abandon its protection of the domestic market and throw its weight, instead, into the building of a European computer industry.

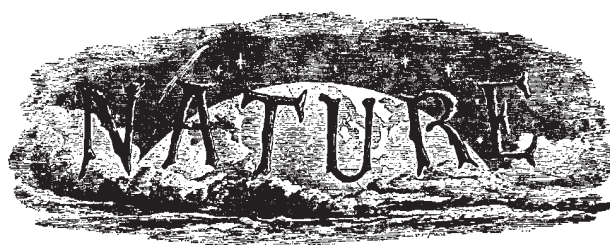
Muddle in America

It is improbable that the American government, the Administration and Congress lumped together, knows how foolish its budgetary procedures make it seem. After ten months of fierce argument, it now seems to have been accepted in Congress that the battle to force an increased rate of spending on the Administration in health and education has been lost. Our Washington Correspondent reports this week (see page 81) that Congress and the Administration have finally reached a stalemate on the Health, Education and Welfare Bill, as a result of which the level of spending during the current fiscal year (with less than four months to run) will be pegged at what it was last year. Among the scientific organizations seriously affected will be the National Institutes of Health, which have in the past eight months been uplifted first by the Administration's own request for a higher rate of spending and then by Congress's show of still greater generosity and finally have found themselves pegged back to a rate of expenditure essentially the same as that specified in the inadequate budgets for 1971-72. The difficulties that this will cause are easily imagined. Having committed themselves to some of the plans drawn up when it seemed that the budget would be generous, the National Institutes of Health will now find it necessary to cut back on other programmes in principle just as worthy. Moreover, it is programmes in principle just as worthy.

Battles over the budget for the Department of Health, Education and Welfare are, of course, a traditional part of the congressional year. Just as the department itself is accurately known as the anthep, so its annual budget creates a greater variety of controversial issues which may be delayed in Congress itself by arguments over questions such as the bus-ing of negro children to white schools and in the White House if Congress should have committed itself to fancy schemes for spending money that the President disapproves of. In the past, the budget of the National Science Foundation has frequently been delayed and even emasculated simply because it is a part of the vast compendium of legislation covering the Departments of Health, Education and Welfare. This year, it is the NIH that suffers. What Congress and the Administration should acknowledge is that capricious accidents like these are not merely a guarantee of inefficient expenditure but also a great source of demoralization among scientific workers. Yet on the face of things, the way out of this kind of difficulty would seem simple enough. Even though the Department of Health, Education and Welfare is a single entity in the government (which should be broken up), is there any reason why the budgets for the several

components of the department should not be separately presented, argued over independently in Congress and then approved (or vetoed, since that seems to be President Nixon's predilection) on their own? In the old days, in the 1960s, American administrators were continually arguing over the best means of constructing a national science policy. They have rightly rejected the idea that there should be an executive department with responsibility in this field, but they should now acknowledge that the repeated muddle over important parts of the science budget is not merely an assurance that there will never be a coherent science policy but that those agencies responsible for executing scientific work will carry the burden for the government's administrative foolishness.

100 Years Ago



Perception in the Lower Animals

As several persons seem interested in Mr. Wallace's suggestion that animals find their way home by recognising the odour of the places which they have passed whilst shut up, you may perhaps think the following little fact worth giving. Many years ago I was on a mail-coach, and as soon as we came to a public-house, the coachman pulled up for the fraction of a second. He did so when we came to a second public-house, and I then asked him the reason. He pointed to the off-hand wheeler, and said that she had been long completely blind, and she would stop at every place on the road at which she had before stopped. He had found by experience that less time was wasted by pulling up his team than by trying to drive her past the place, for she was contented with a momentary stop. After this I watched her, and it was evident that she knew exactly, before the coachman began to pull up the other horses, every public-house on the road, for she had at some time stopped at all. I think there can be little doubt that this mare recognised all these houses by her sense of smell. With respect to cats, so many cases have been recorded of their returning from a considerable distance to their homes, after having been carried away shut up in baskets, that I can hardly disbelieve them, though these stories are disbelieved by some persons. Now, as far as I have observed, cats do not possess a very acute sense of smell, and they seem to discover their prey by eyesight and by hearing. This leads me to mention another trifling fact: I sent a riding-horse by railway from Kent *via* Yarmouth, to Freshwater Bay, in the Isle of Wight. On the first day that I rode eastward, my horse, when I turned to go home, was very unwilling to return towards his stable, and he several times turned round. This led me to make repeated trials, and every time that I slackened the reins, he turned sharply round and began to trot to the eastward by a little north, which was nearly in the direction of his home in Kent. I had ridden this horse daily for several years, and he had never before behaved in this manner. My impression was that he somehow knew the direction whence he had been brought. I should state that the last stage from Yarmouth to Freshwater is almost due south, and along this road he had been ridden by my groom; but he never once showed any wish to return in this direction. I had purchased this horse several years before from a gentleman in my own neighbourhood, who had possessed him for a considerable time. Nevertheless it is possible, though far from probable, that the horse may have been born in the Isle of Wight. Even if we grant to animals a sense of the points of the compass, of which there is no evidence, how can we account, for instance, for the turtles which formerly congregated in multitudes, only at one season of the year, on the shores of the Isle of Ascension, finding their way to that speck of land in the midst of the great Atlantic Ocean?

CHARLES DARWIN

From Nature, 7, 360, March 13, 1873.

OLD WORLD

Select Committee as a Nuclear Engineer

MARKED enthusiasm for the high temperature reactor and a distinct lack of interest in the steam-generating heavy water reactor characterized the evidence of BNDC when the group appeared before the Select Committee on Science and Technology last week. British Nuclear Design and Construction is one of Britain's two nuclear consortia, the other being the Nuclear Power Group, and it is clear that their interests do not coincide.

The Nuclear Power Group was obviously eager to build a SGHWR when it gave evidence to the select committee recently (see *Nature*, 241, 301; 1973), but BNDC told the committee that although the consortia has a "high opinion of the reactor's technical merits", it does not think there is much chance of selling it abroad, partly because it is "too like the light water reactors in many respects".

The high temperature reactor, according to BNDC, is another matter altogether. The first HTR could be operating in Britain by 1980, and the consortium believes it "holds out the prospect of catching up on the success of the light water reactor". Interest in France, West Germany and the United States is already high, and Gulf General Atomic of the United States has already had orders for 6 HTRs even though the company's 300 MW prototype will not be finished until the end of the year.

BNDC has already had discussions with Gulf on high temperature reactor designs, the consortium told the committee. The interest being shown in France and West Germany, allied to Britain's technical knowledge, clearly hints at the possibility of international collaboration.

But if BNDC's ideas on what type of reactor should be built to follow the as yet uncommissioned advanced gas cooled reactors differed from TNPG's, so did their ideas on the reorganization of the two consortia that Mr John Davies promised last August.

TNPG's chairman, Sir Edwin MacAlpine, told the Select Committee that it "would be a terrible disaster if any one company were seen to be in control" of the new design and construction company, but BNDC saw nothing wrong with an arrangement that resulted in the new company being controlled by a management contract. The rumour is that Sir Arnold Weinstock, chairman of GEC, is the man to whom

the government would like to award that contract. GEC already manages the financial side of BNDC, and Sir Hector McNeil, BNDC's chairman, was, perhaps not surprisingly, enthusiastic about the arrangement. "I think it would assist" in the running of a new company, he said. He also argued that the new consortia should be owned 51 per cent by industry, and 49 per cent by government.

But if Sir Hector fanned the select committee's fears that Sir Arnold Wein-

stock may become the doyen of Britain's nuclear construction industry, he at least calmed fears that Britain is about to buy American light water reactors, the safety of which has been much questioned of late. "I see no reason at this stage to abandon the advanced gas cooled reactor for light water reactors," he said.

Later in the week the Select Committee took evidence from Mr Christopher Layton, *chef de cabinet* to Mr Alterio Spinelli, the member of the

ENVIRONMENT

Conservation Review Complete

THE Nature Conservancy's conservation review, started in 1967, has been completed but is unlikely to be published before early 1974.

The object of the review—which has been approved by the Natural Environment Research Council but which has yet to receive the seal of the Department of Education and Science and the Department of the Environment—is to identify sites in Britain that are of national importance.

The review itself is a strictly scientific document but the object that lies behind it is to produce a coherent national conservation policy in which it is hoped government departments, local authorities, public and private landowners and industry will cooperate.

The review consists of two parts. The first is a description of the chief habitat types found in Britain and their variations. Six chief types of habitat are listed, coastland, woodland, grassland, open water, uplands and peatlands. In the second section, particular sites around Britain are described and classified into one of two grades, both of which are classified as being of national importance (the Nature Conservancy places other sites into three or four further grades of regional and local importance).

These sites are graded by applying criteria which take into account the size of the site, the diversity of its habitat, its rarity, fragility, representativeness, research and educational value, its known history, the extent to which it has been modified and its potential value if it has been damaged but is restorable. More than 400 sites are believed to be covered in the review and classified as being of national importance.

The field-work for the review was carried out by both the conservation and research sides of the conservancy. Conservation officers in the regions were best informed about known sites, but the scientific staff identified a number of others, particularly in the remoter parts of the country.

It is not intended that the review should form a once-and-for-all assessment of Britain's most important natural habitats. New sites can be added as they are identified.

The considerable delay in the publication of the review can be attributed to the government's white paper on research and development published last July. The conservation side of the Nature Conservancy is to be separated from the research side and transferred to the Department of the Environment. So much time has been spent preparing for this change (which has not yet taken place as legislation is needed) that the review has had to be put back. This has caused a certain amount of dissatisfaction in the regions as Nature Conservancy staff were working hard against a deadline eighteen months ago to finish a review that will not now see the light of day until next year. The fault is not the conservancy's. The problem lies chiefly in having to prepare for major changes and being at present answerable to two masters, the old (DES) and the new (DOE), and having to work out procedures with the latter. The DOE inevitably has to be involved although the review is purely a scientific assessment of known sites, because its policy implications are considerable. The simple publication of the document will, in itself, be a pressure for a conservation policy.

European Commission responsible for industry and technology.

Outlining the makings of a European computer policy, Mr Layton told the committee that although IBM dominates the European computer market there are specific areas where IBM can be beaten by European companies. A large company like IBM needs disciplining by effective competition, he said, and Europe must provide this, although the United States and Japanese markets will have to be penetrated if a large European computer company is to become viable.

The five leading companies in Europe are ICL, Philips, Siemens, CII and AEG-Nixdorf. Philips, Siemens and CII have already discussed the possibilities of developing a new range of computers together, and Mr Layton suggested that ICL and AEG-Nixdorf would also make a good partnership. "If ICL can find a European partner with whom it can merge effectively the commission would warmly welcome it."

Mr Layton also discussed support given the national computer companies from public funds in Britain, France and West Germany. Such support is clearly against the spirit and letter of the Treaty of Rome, he said, but "in fact the aids are tolerated by the commission and protective national procurement practices continue at the present time". This situation cannot continue indefinitely and the question is whether the national preferential policies are to be replaced by a community preferential policy—at least for a time. Massive government support in the form of orders accounts for IBM's success in the United States, and without this support Mr Layton does not believe the European national companies would be viable.

ESRO

Choosing the Next Step

from a Correspondent

THE European Space Research Organization is to decide by the end of this month which satellite to launch later this decade to follow COS-B and GEOS. The three options—only one of which will be chosen this month although the others may be added later—were presented at a lively symposium in Frascati last week to a wide selection of European scientists.

The Venus orbiter would investigate the atmosphere and surface of Venus, HELOS (Highly Eccentric Lunar Occultation Satellite) would provide observations of X-ray sources, and the mother-daughter projects would study the earth's magnetosphere and the adjacent regions of interplanetary space.

The Venus orbiter mission would link up with NASA's proposed biennial Venus project which includes a probe

that will explore the Venusian atmosphere. The orbiter would be provided by ESRO and the experiments selected by a joint NASA/ESRO team. Venus is of particular interest to geophysicists as its mass and radius are the closest of all the planets to those of the Earth. It is also hoped that studies of the comparatively simple circulation patterns in the atmosphere of Venus will lead to a greater understanding of the more complex circulation patterns on Earth.

The original concept of the HELOS mission (see *Nature*, 228, 756; 1970) used moon occultations to obtain positions and structures of X-ray sources with high precision. An off-set mode now offers the additional capability of making continuous observations over periods of many hours. Variable X-ray sources can thus be observed from a satellite, which because of its eccentric orbit, does not suffer from the frequent interruptions of the radiation belt and Earth occultations as it would be in a near Earth orbit. HELOS is a purely European project complementary to NASA's X-ray programme. However there are now difficulties in the funding of NASA's high energy astronomical observatory in the late 1970s.

The mother-daughter project involves the launch of two satellites on one Thor Delta rocket. They will be launched in conjunction with NASA's IMP programme to combine with its heliocentric satellite which will be in solar orbit close to the libration point at 250 Earth radii. These missions should increase the understanding of the physics of the magnetized plasma in which the Earth is embedded. The multiple satellite mission will help unravel the spatial and temporal variations of the radiation belt, the magnetopause, the bow shock and magnetotail. When the mother and daughter are within the magnetosphere the heliocentric spacecraft will act as the monitor of the solar wind and thus establish the relationship between such phenomena as substorms with changes in the solar wind.

The launch dates for the three satellites if they are chosen later this month are mid-1977 for the mother-daughter satellite, mid-1978 for the Venus orbiter and the end of 1977 for HELOS. Estimates of the costs of the three satellites are not yet available.

COMPUTERS

No Freeze

THE Computer Board for Universities and Research Councils spent £9.3 million in 1971-72, a twenty-six per cent increase over the previous year. Not only did the expenditure go up by this amount but the spending in 1971-72 was almost £200,000 less than the board allocated for the year. Of this total

£6.23 million was spent on hardware, £2.7 million on recurrent costs and only £0.38 million on buildings (Cmnd. 5220, HMSO, £0.16).

Computer capacity in British universities is now ten times greater than it was in 1965, and the board estimates that by March 1974 more than twenty-five times the 1965 capacity will be available—a growth rate of about 40 per cent a year. But the board says that this increase is only a lower estimate of the available running time chiefly because the quality of computer performance has increased during the past decade and users now have better access to the facilities.

During the past two years, the Computer Board has authorized the buying of eighteen new computers of which twelve have already been delivered. The new installations include the CDC 7600 at the University of London which is also providing facilities for other universities in the south-east of England, the CDC 7600 and its linked partner the ICL 1906A at the University of Manchester, the 1906A computers installed at the Universities of Birmingham, Leeds, Nottingham and Oxford and the IBM 370/165 at the University of Cambridge. The Cambridge machine is the only IBM machine installed recently, and the board says that the

Support a Tree

Two £50 prizes are being offered by the Crown Estate Commissioners for the best designed tree support and guard. This exercise, which coincides with Plant a Tree Year 1973, will ensure that trees planted this year will have the maximum chance of survival.

The commissioners are looking for two types of support/guard one primarily for local use, a do-it-yourself product that would be manufactured from timber or other easily accessible materials, and a second type that could possibly be manufactured and widely sold.

The closing date for the competition is May 21, when plans must be submitted to the Crown Estate Office. But by June 18 a model of the support/guard has to be delivered to the Royal Show at the National Agricultural Centre at Stoneleigh in Warwickshire.

The support/guard has to achieve two objectives. First it must support a tree until it can establish itself and second it must be proof against rabbits, hares and stock for not less than ten years.

choice was influenced to a great extent by the Cambridge argument that it needed a computer that would be compatible with the many overseas universities and research institutes which have equipment—so that computer programmes could easily be interchanged—and also so that the system would be compatible with the IBM facilities at the Medical Research Council's Molecular Biology Laboratory at Cambridge. As its part of the bargain, the MRC agreed to contribute £160,000 to the capital cost and £20,000 a year to the recurrent costs of the installation in return for ten per cent of the running time. Also, the Cambridge machine will be available "for a substantial proportion of time" to users from other universities who need time on an IBM machine. The board states that the arrangements for such allocations are under consideration although twenty per cent of the time has been set as a tentative limit.

Since the Computer Board was set up in 1966 it has spent £18.8 million on ICL computers, £4.95 million on IBM computers, £9.0 million on CDC computers, £290,000 on DEC machines and £150,000 on computers from other manufacturers. By far the biggest sum spent on IBM machines has been the £1.75 million spent on the 370/165 at Cambridge while the CDC 7600 at the University of Manchester cost £2.23 million. The largest single contract for an ICL machine was also placed at Manchester for the 1906A, the linked partner of the CDC 7600.

Five ICL 1906A computers have been provided by the Computer Board in the past two years and the users of these machines, together with ICL, have set up a Software Steering Committee to identify developments required to improve the performance of the computers, and subsequently to implement the developments. These universities have also, in collaboration with the Science Research Council's Atlas Computer Laboratory at Chilton, set up a library of mathematical routines. Since the first edition of this library was released in October 1971 the board has encouraged the group to make the library more generally available. The eventual aim of the group and the board is to establish a master library which is independent of the machine.

WORLD HEALTH ORGANIZATION

Problems of Age

Two recent reports from the World Health Organization highlight the need for treatment for diseases which are on the increase in the world today. In the first report, *Drug Therapy for Cancer*, by G. Brule, S. J. Eckhardt, T. C. Hall and A. Winkler, the authors point out that in many parts of the world surgical

and radiotherapeutical methods of treating cancer are simply not available and treatment by drugs, chemotherapy, is therefore becoming much more important. In the other report, *Psychogeriatrics* (WHO Technical Report No 507), a warning is given that the increased longevity of people is leading to an increase in mental diseases but that not enough is being done to cater for these people.

The ever-increasing presence of carcinogens in the environment combined with a population that is becoming older means that cancer will be an increasing problem, in spite of the fact that effective treatment for many forms of the disease is now available. But in the developing countries there are few facilities for treatment except by giving drugs. The role of chemotherapy is therefore of greater importance in these countries than it is in the developed countries where other forms of treatment are available. It is fortunate that the treatment of cancer by drugs is one of the great developments of the past fifty years.

At present hundreds of drugs are tested every year to see whether they are suitable for use in combating tumours. The introduction of community programmes to screen for certain diseases has meant in recent years that effective drugs can be identified much more easily than in the past. Of the many drugs tested, however, only a few have been accepted and the report pleads that doctors and medical health advisers in countries where chemotherapy is the only possible method of treatment should be made aware of what drugs are available and how efficient they are in treating cancer.

The report includes a documentation of drugs that are available and an account of their efficacy and how they should be administered. The emphasis in the report is on the practical side of treatment for cancer and the World Health Organization hopes that as well as being suitable for both undergraduate and postgraduate education the report will prove helpful to the practitioner who is faced with difficult problems of what particular drug to prescribe for treating cancer.

The second report, although on a different disease, has also arisen chiefly because of the increasing longevity of people. An increased expectation of life means an increased expectancy of mental illness, which becomes progressively more common as people grow older due in part to detrimental changes in the brain and its blood vessels.

The report, however, is written in a partial vacuum in that there is an inadequacy of epidemiological data of psychiatric diseases of old people. The report urgently calls for more data and

also for a study of currently available services for treating the mentally ill and the pattern of use of such facilities.

In the past, according to the report, many psychiatric illnesses of old age have not been recorded as such and so the magnitude of the problem has, in general, been underestimated with only the most severe psychoses and dementia being noted.

As part of the plan to study the problem the report recommends that an operational research exercise be carried out to define the merits of keeping old people in institutions compared with keeping them at home or in community centres. In such a study the effects on the other members of the family should be taken into consideration. It is also pointed out that early screening programmes could help in alleviating the difficulties of solving problems associated with old people whose mental health has failed.

Royal Society Exchange

SINO-BRITISH scientific collaboration is going from strength to strength. Later this month, three British scientists will visit China for between two and three weeks, a visit which reciprocates that made to Britain last October by a seven man delegation of Chinese scientists (see *Nature*, **239**, 245; 1972).

The present round of visits was initiated in May 1972 by Sir Alan Hodgkin, President of the Royal Society, who went to China with Sir Kingsley Dunham and Sir David Martin. Until then there had been no regular visits between the countries since the cultural revolution of the mid 1960s.

The present visits will be undertaken by Professor R. L. Wain of the agricultural chemistry department at Wye College, University of London, Dr F. L. Rose of the pharmaceutical division at Imperial Chemical Industries and Professor G. Allen of the chemical physics department at the University of Manchester.

During their stay in China, Professor Wain and his colleagues, who will travel independently and spend differing times in the country, will follow programmes arranged for them by the Academia Sinica. They will visit laboratories in Peking, Shanghai and Canton, and will lecture as well as discuss their work with Chinese scientists. The visits will coincide with the British Industrial Technology Exhibition in Peking.

NEW WORLD

Congress Backs Down on NIH Budget

by our Washington Correspondent

CONGRESS has finally given up its attempts to pass an appropriations bill for the Department of Health, Education and Welfare which is acceptable to President Nixon. With only four months of the present fiscal year left to run, both the House and the Senate last week reluctantly decided to extend until June 30 the arrangements under which HEW has been operating for the past eight months. The upshot is that the Administration will simply fund the agencies of HEW, which include the National Institutes of Health, at a level considerably lower than Congress wishes.

The long struggle between Congress and the White House over the HEW budget is just one facet of the bitter dispute which is now raging over which branch of government should have final control over the pursestrings, but it is a struggle which particularly concerns the scientific community since the HEW budget contains the bulk of the federal government's expenditures on biomedical research.

Last year, the HEW budget became an election year political football, with Congress twice passing measures which President Nixon vetoed as inflationary. Finally, in the dying days of the last session of Congress in October, a continuing resolution was passed which allowed HEW to receive funds until February 28. The idea was that when Congress reassembled in January, it would have another crack at drawing up a budget for the department which would either be acceptable to President Nixon in his present parsimonious mood, or which would have sufficient support on Capitol Hill to allow a presidential veto to be overridden.

But last week, Congress backed down and simply extended the continuing resolution for the rest of this fiscal year. Explaining why the appropriations committees decided not to try for another bill early in this session, Mr Daniel Flood, chairman of the appropriations subcommittee which deals with the HEW budget said "frankly, our committee believes that an attempt to enact a third bill would be a wasted effort. It seems very unlikely that a bill which would be acceptable to a majority of the Congress would also be acceptable to the President". He also pointed out that it is now time for Congress to start work on the 1974 appropriations bill.

The continuing resolution, in short, provides authority for the Administration to spend money on HEW programmes up to the lowest level for individual items contained in either of the separate appropriations bills passed by the House and the Senate last June. The resolution thus allows the Administration to spend up to about \$28,000 million this fiscal year through HEW. But even this amount, which is some \$600 million less than the first vetoed bill entailed, is still more than \$1,000 million greater than President Nixon's original budget request of \$26,800 for HEW. Moreover, at the end of last year, when Congress was trying to ram through an appropriations bill increasing President Nixon's budget request, the Office of Management and Budget was busy revising the request downwards, and in January this year the

OMB proposed a revised budget for HEW of \$26,100 million.

The irony of the whole business is that it makes no difference what level of funding Congress finally decided upon, since the Administration will spend only as much as it wishes. In other cases in which Congress has tried to increase budgets this fiscal year, the Administration has simply impounded some of the appropriated funds and Senator Norris Cotton of New Hampshire, speaking for the Administration last week, said that "if this continuing resolution should pass in its present form, it is the purpose of the Department of Health, Education and Welfare, for the remainder of the year, to make its expenditures on the basis of the President's revised budget of \$26,100 million". That, of course, is what Congress is getting angry about.

UNIVERSITIES

Free Speech in California

by our Washington Correspondent

CHARLES SCHWARTZ, a professor of physics at the University of California at Berkeley, has won a lawsuit against the Lawrence Berkeley Laboratory on the grounds that he has been refused summer employment because of his radical political activities. The court ruled that such activities are constitutionally protected, and directed the laboratory to compensate him for loss of salary. The ruling is the latest, and perhaps the final, step in a dispute between Schwartz and the laboratory's management over the question of whether scientists should be allowed to take part in political activities on the laboratory's premises in their free time.

The issue was first raised in the autumn of 1969, when political activism was at its height on university campuses throughout the United States. At that time, a group of scientists at the laboratory proposed holding lunchtime political discussions about the Vietnam war, but their proposal was vetoed by Dr. E. M. McMillan, the laboratory director. McMillan later appointed a committee to examine the laboratory's policies with respect to political activities on the campus, and in March the following year, the committee backed McMillan's actions in excluding political meetings from the laboratory.

Later that year, however, Schwartz, who was employed by the laboratory during the summer, held two lunchtime seminars in defiance of the ban on meetings, and was promptly suspended for two weeks, a sentence which was later reduced to two days. The following year, Schwartz was informed that his request for summer employment had been denied, and he immediately filed complaint with the faculty Committee on Privilege and Tenure. When the committee failed to find fault with McMillan's action, Schwartz appealed to the Academic Freedom Committee, the President of the University and the Board of Regents, all to no avail. Finally, the local branch of the American Federation of Teachers took the case to court, the upshot of which was Schwartz's victory last week. While the legal dispute was going on, however, McMillan announced a new policy at the laboratory allowing complete freedom of speech.

Schwartz, who is very much a radical thorn in the University of California's flesh, promptly accused the administration of the University of California at Berkeley of not living up to its dedication to free speech by backing up his fight with the laboratory management. His victory may, however, be shortlived, because the court ruling included the statement that "this decision should not be construed as affecting future summer job placement opportunities for petitioner which may be dictated by budgetary considerations".

What does all this mean for NIH? The short answer is that every institute will have a smaller budget this year than last, apart from the National Cancer Institute and the National Heart and Lung Institute, which will get modest increases. Moreover, there will be large cutbacks in health manpower education and training. In contrast, President Nixon's original budget request, made before the cutbacks in overall federal spending late last year, would have increased the budgets of all agencies, and the vetoed bills would have provided them with a veritable bonanza.

One effect of the financial stringencies in NIH is that the amount of money available for new grants will be particularly tight. According to an item in the newsletter *Science and Government Report*, many of the NIH non-competitive grants—those used to fund projects over several years—will be cut back to make more money available to support new projects. The result will be that researchers who have embarked on long-term projects with NIH funding will be given less money than originally promised.

President Nixon's budget request for next year entails even less expenditure for many HEW programmes—including most institutes in NIH—than his revised budget for this year. There is thus clearly going to be another protracted fight between Congress and the White House over funds for health and social programmes. If this year's fiasco is any guide, however, President Nixon is likely to get his way in the end.

BUDGETS

Light in the Gloom

by our Washington Correspondent

AMID the gloom and despondency in the scientific community following publication of the Administration's austere budget for 1974 and the dismantling of the White House science policy machinery, a report published by the Battelle Memorial Institute sticks out like a sore thumb. The institute reckons that total expenditures on science and technology in the United States will amount to about \$30,100 million in the calendar year 1973, which would represent an increase of 7.5 per cent compared with estimated outlays in 1972. The forecast predicts that the chief increase will come from industry, but it also suggests that "Federal support has begun what could be a sustained rise".

The institute's optimism will certainly be regarded with some surprise by those scientists who have had their budgets cut or their projects terminated as a result of cutbacks in spending by the Administration, but the report nevertheless suggests that federal spending

on science and technology will climb to about \$16,300 million this year, compared with \$15,200 million last year; spending by industry is predicted to increase from \$11,320 million last year to about \$12,200 million this year. The remainder will come from academic and non-profit institutions, which are expected to increase their expenditures on research and development slightly.

The forecast for federal spending is based on recent trends in budgets, which the report suggests is "somewhat safer than reliance on recently published figures". Nevertheless, the most recently published figures—the Administration's budget proposals—suggest that federal spending on science and technology in the 1973 fiscal year (which runs from July 1972 to June 1973) will be about \$15,900 million, an absolute decline of some \$200 million from the last fiscal year. Since the Battelle forecast is for the 1973 calendar year, this sharp but perhaps temporary cutback in overall federal spending in the 1973 fiscal year does not show up in the optimistic figures given in the report.

As for patterns of expenditure, the forecast notes that there has been a shift away from military, nuclear and space research towards civilian science and technology, but it also warns that "in longer term overall dollars, this shift is still not as significant as many believe". The predicted increase in industrially funded research and development follows partly from an expected increase in corporate sales.

A year ago, the Battelle Memorial Institute predicted that total expenditures on science and technology would begin to increase after a period of stagnation, but warned that the full impact of the upturn might be delayed. That, indeed, is what has happened—the institute predicted last year that spending on research and development would reach \$30,100 million in 1972, but it actually reached only about \$28,000 million, and the institute is now repeating for 1973 the prediction it made for 1972.

SCIENCE POLICY

Nixon Gets His Way

by our Washington Correspondent

PRESIDENT NIXON now seems almost certain to get away with his plans for scrapping the Office of Science and Technology without running into opposition from Congress. Hearings held independently by House and Senate Government Operations subcommittees last week brought out the fact that isolated outbreaks of sharp words and the general air of disquiet among many scientists over the plans do not give the committees sufficient reason to be difficult about them. Congress has until April 6 to pass a resolution disapproving

the plans, but so far, no disapproving resolution has been introduced.

The hearings last week were chiefly a formality, and provided a forum for Fred Malek, deputy director of the Office of Management and Budget, and Dr H. Guyford Stever, director of the National Science Foundation, to reiterate the Administration's thinking. The reorganisation plan (see *Nature*, 241, 234; 1973) involves scrapping the Office of Science and Technology and the post of Science Adviser to the President, and transferring to the Director of the National Science Foundation some of the responsibilities for advising the president on science policy.

Both Senator Abraham Ribicoff, chairman of the Senate Government Operations subcommittee, and Chet Hollifield, his counterpart in the House of Representatives, noted that the plan has been criticised as downgrading scientific advice in the Administration—Dr Stever, unlike previous Science Advisers to the President, will advise not the president, but Dr George Shultz, Secretary of the Treasury and Presidential Assistant for economic affairs. Malek replied that the National Science Foundation has more staff to perform science policy analyses than could possibly exist in the White House, and that "Dr Stever will have ready access to the President's closest advisers."

Apart from the question of downgrading advice, members of the House subcommittee were a little concerned that no increase in budget or staff has been given to the National Science Foundation to enable it to perform its new role, but Malek simply said that it is his feeling that "the new NSF budget, with the number of people and resources that they have, will be sufficient to absorb this added capacity". In other words, the foundation will have to carry out its new responsibilities with money and people originally required for other purposes.

The most outspoken criticism of the plans last week came from Mr John Davis, chairman of the House Subcommittee on Science, Research and Development. In a statement presented to the Government Operations subcommittee, Davis expressed alarm at the scrapping of OST and concern about the present state of US science and technology in general. He pointed out that the United States has a balance of payments deficit in high technology products, and that in terms of percentage of gross national product, the US spends about half as much on research and development as Japan and West Germany, the two chief exporters of high technology products to the United States. In such a situation, Davis suggests that "the abolition of OST and a downgrading of our scientific apparatus seems counterproductive".

NEWS AND VIEWS

T-Mycoplasmas and Reproductive Failure

ATTENTION was drawn recently (see *Nature*, **241**, 425; 1973) to the significant relationship between maternal infection and foetal loss and morbidity. Rubella, cytomegalic inclusion disease and toxoplasmosis were all cited as causing retardation of human foetal growth and reference was made to further evidence, pointing to the possible implication of Coxsackie viruses in retardation of foetal growth and certain congenital malformations, particularly those affecting the heart. Low birth weight has been attributed to human maternal infection with T-mycoplasmas (Braun and colleagues, *New Engl. J. Med.*, **284**, 167; 1971), and these organisms have been isolated from the foetal membranes in cases of repeated abortion (Kundsinn and colleagues, *Science*, **157**, 1573; 1967).

Mycoplasmas, which used to be known as pleuropneumonia-like organisms, are the smallest free-living organisms, with similarities to both viruses and bacteria. They resemble viruses in size, and are therefore ultrafiltrable, as well as in having no rigid cell wall. They resemble bacteria in their ability to grow in acellular media; in their chemical composition which includes both RNA and DNA and in their growth being inhibited by tetracyclines and some other antibiotics which interfere with their metabolism. A feature of the T-strains is their ability to metabolize arginine or urea with the production of ammonia (see *Lancet*, ii, 248; 1967). Mycoplasmas have been implicated in a wide variety of human diseases including primary atypical pneumonia (Channock and colleagues, *Proc. US Nat. Acad. Sci.*, **48**, 41; 1962), myringitis, otitis media, various rashes, erythema multiforme, the Brodie-Reiter syndrome and, in the case of the T-strain, non-specific urethritis.

On page 120 of this issue of *Nature*, H. Gnarpe and J. Friberg of the University of Uppsala present evidence that T-mycoplasmas may be a possible cause of human reproductive failure. All who have had experience of the diagnosis and treatment of human infertility are well aware of the not infrequent couples who, after full conventional investigation, fail to present any explanation for their continued reproductive failure. Emotional factors, immunological causes and mysterious "infections" are among the possibilities that the groping clinician postulates as a cover-up for his ignorance and his inability to account satisfactorily for the continuing infertility.

It is just such a group of patients that Gnarpe and Friberg have studied, having eliminated the common causes of reproductive failure by conventional clinical investigations. They were able to isolate T-mycoplasmas from 89 per cent of the hundred and four patients (from the seminal fluid of forty-five of the husbands and from the cervical mucus of forty-seven of the wives). By contrast, T-mycoplasmas were isolated from the cervical mucus of only nine of forty pregnant women and from the seminal fluid of six of twenty-three men married to women who were pregnant or who had recently delivered. The difference in the incidence of T-mycoplasmas between the

fertile and infertile patients is highly significant. Treatment with doxycycline was shown to eliminate T-mycoplasmas from all of ten men and nine of eleven women. Within five months of eradication of T-mycoplasmas 29 per cent of the infertile women had conceived, no pregnancies having occurred in a preceding 3-months' period of observation.

These findings are of great interest and would certainly seem to justify the further investigations which the authors are carrying out. Perhaps two comments might be justifiable. In the first place, it is possible that the much lower incidence of T-mycoplasmas found in pregnant women and their husbands could be attributable to the pregnancy—and it should not be difficult to refute this suggestion if untrue. In the second place, the restoration of fertility by treatment could have been more convincingly demonstrated if a double blind trial of therapy had been carried out, because it is not impossible that psychotherapeutic factors may have been involved.

G. I. M. S.

Ubiquitous Neutrinos

DURING the past year neutrinos seem to have become a panacea for many of astronomy's ills. So little is known about the physical properties of these elusive particles that, as many astrophysicists (and geophysicists) are coming to realize, the visible manifestations they produce in different models can be adjusted to cover a multitude of sins by suitable tinkering with, for example, the hypothetical mass and equally hypothetical lifetime of the neutrino.

In January last year Bahcall, Cabibbo and Yahil published a discussion of the stability of the neutrino in which they pointed out that "our present knowledge of neutrinos is insufficient to establish whether or not neutrinos are stable" (*Phys. Rev. Lett.*, **28**, 316; 1972). The point, of course, is that the very low counting rate reported from solar neutrino detectors implies that neutrinos are very stable particles. If they have finite mass they could be unstable, and Bahcall *et al.* showed that this could still be consistent with the solar neutrino experimental results.

But what is really meant by stability in this case? All that is really "known" from those experiments is that a solar neutrino, with typical energy 1 MeV, lives long enough to travel the distance between the Earth and Sun at the speed of light—that is, for 500 s. But even this evidence could be the result of misinterpreting the data. According to Bahcall *et al.*, standard astrophysical models of the Sun could be incorrect, so that neutrinos are not produced in the amounts predicted. Alternatively, the solar models may be correct, but the neutrinos produced decay before they reach Earth. Either effect could explain

the discrepancy between theory and observation. Several articles published in *Nature* and *Nature Physical Science* recently have explored the consequences of the first proposal, suggesting, among other things, that repeated sudden mixing in the Sun's core could be associated with the temperature changes that produced ice ages on Earth.

But what of the second possibility? Once it is assumed that neutrinos from the Sun do decay, the flux of neutrinos near the Earth is a very sensitive function of energy, and there are even more parameters which can be adjusted. Hamza and Beck have suggested (*Nature*, **240**, 343; 1972) that the adjustments allowed might be drastic enough to encourage speculation about the role of neutrinos produced by radioactive decays within the Earth itself.

Experiments reveal that the neutrino is stable over about 10^4 cm, and if the solar neutrino decay hypothesis is correct the neutrino decays within about 10^{13} cm. By choosing a range of 100 m, energy from radionuclide decay in the form of neutrinos is certainly kept within the Earth's crust instead of being lost; a stable path length of 300 km is better from a geophysical point of view, say Hamza and Beck, because the energy from the neutrinos would be released in the form of heat at a range of depths around 300 km, where there is "a well known second order seismic discontinuity", hitherto unexplained.

The latest development in the speculative flight of neutrino fancy comes from Cowsik and McClelland, who follow up the implications of the positive mass required to allow neutrinos to decay (*Astrophys. J.*, **180**, 7; 1973). They consider the evolution of a big-bang universe in which many such neutrinos were produced before the time when radiation and matter decoupled. Unlike other particles, neutrinos to a large degree survive annihilation as the Universe expands, because of their small cross-sections for interaction. The number density decreases only with increasing volume of the Universe, say Cowsik and McClelland (who seem to be trying to both have their cake and eat it, for their neutrinos certainly do not decay in 10^2 cm, or even 10^{13} cm; but that is quite within the rules of the neutrino game as long as one does not pretend to solve all astrophysical problems at once). A mass for the neutrino of $2.5 \text{ eV}/c^2$ is well within the best experimental limit yet obtained ($<60 \text{ eV}/c^2$), and according to Cowsik and McClelland this would be sufficient for the gravitational influence of the sea of neutrinos throughout space to close the Universe.

That is a happy thought for those cosmologists who would like the Universe to be closed. But it is possible to go still further. With a neutrino mass of a few eV/c^2 neutrinos would be by far the dominant gravitational influence in the Universe. Could the growth of galaxies and clusters of galaxies have been triggered by gravitational interactions of neutrino clouds? There is certainly a long-standing discrepancy between the masses needed to bind many clusters of galaxies together and the total mass obtained by summing the masses of individual galaxies, estimated according to the best astrophysical theories.

Cowsik and McClelland have looked in detail at the case of the Coma cluster, a well known example of the "missing mass" dilemma. Treating neutrinos as a Fermi-Dirac gas now at zero temperature, the mass of each neutrino in the cloud must be about $2 \text{ eV}/c^2$, with a total mass of 4×10^{49} g, if the cluster is to be bound. The distribution of mass within the resulting potential well happily fits the observed central peak and extended tail seen in the distribution of

visible galaxies in the Coma cluster; as such a well develops, whatever its origin, neutrino quantum states are progressively drawn in so the neutrinos fill the well. Neutrinos of mass 2 to $3 \text{ eV}/c^2$ can, it seems, close the Universe and bind the Coma cluster.

Should all this be taken seriously? Certainly not. After all, the various intriguing neutrino models are not consistent with one another. Whether one of the suggestions should be taken seriously is another question, to be answered, perhaps, in accordance with whichever problem seems the most baffling. With neutrinos, it seems, one can solve some of the problems most of the time, and most of the problems some of the time. But it does not seem possible to solve all of the problems all of the time. Alas, the panacea is not universal after all.

By our Cosmology Correspondent

Drift in the Ionosphere

IF the physical and chemical parameters of the F region of the Earth's atmosphere could be measured more accurately, understanding of it would increase enormously. A great many processes affect this region and the task of sorting these out is hampered by the large uncertainties in the measured parameters and also the striking lack of data. One of the chief examples of this is the measurement of wind velocities in the F region. Many experiments measure only a single component of the velocity whereas the accurate measurement of the components parallel and perpendicular to the Earth's magnetic field are vital to the discovery of the underlying causes of these winds.

The F region lies between heights of 170 and 1,000 km and is the most densely ionized of the ionospheric layers, having an electron density of 10^{12} m^{-3} . It is formed by the ionization of atomic oxygen by solar X-rays. One of the peculiarities of the F region is known as the geomagnetic anomaly. Unlike the lower C, D and E regions the electron density at a specific height in the F region is not constant but varies as a function of geomagnetic latitude. Also the maximum electron density does not seem to be related to the angle of arrival of the Sun's rays as is the case for the other layers. Many suggestions have been put forward to explain these anomalies. Perhaps the loss rate changes because of variations in the concentration of atomic oxygen; alternatively fluctuation in the upper atmospheric temperature may affect the reaction constant; or maybe energetic particles leaking from the magnetosphere produce ionization at certain latitudes; or possibly the anomalies are caused by a movement of the ionospheric plasma, this being caused by winds in the neutral atmosphere and electric fields originating lower in the ionosphere. Now the observed anomalies could be caused by one or more of the above phenomena but there seems to be little doubt that bodily movements of the atmosphere are responsible for part of the anomalous behaviour. The careful study and measurement of the magnitude and direction of these winds are, therefore, very important.

Three methods are being used at present. The chemical trail method relies on a rocket to release a chemical, usually barium or sodium, into the ionospheric region. The

drifting of the resulting chemical trail is then observed from the ground. This technique has certain drawbacks—the trail only lasts for a few minutes, the experiment can only be carried out on clear nights, the maximum height is 200 km and, finally, rockets are expensive. The results, though, are unambiguous and accurate.

A close-spaced antenna system can also be used to measure ionospheric drifts. The ionosphere is illuminated with radio waves from the ground or from a geostationary satellite or a radio star and the diffraction pattern of the reflected or transmitted waves on the ground is then monitored. Changes in this pattern are related to the ionospheric drifts. Many problems, however, arise in the interpretation of the changing patterns and it is now thought that the results only refer to moving fluctuations in the electron density and not to movements of the whole ionospheric region.

A new and very exciting method of measuring ionospheric winds uses observations of incoherent scattering from electrons. An electromagnetic wave falling on a free electron causes it to oscillate and thus to radiate a scattered wave in all directions. This last sentence immediately reveals two of the problems encountered when using this Thomson scattering method to probe the ionosphere. First, electrons are very small (they have scattering cross-sections of 10^{-28} m^2) so very little power is reflected from an ionized layer. For example a scattering volume with the dimensions of a cube of sides 100 km, centred on the maximum of the F layer where the electron concentration is about 10^{12} m^{-3} , has a total scattering cross-section of about 10^{-1} m^2 . It is the power returning from this effective area, placed at a distance of several hundred kilometres, that has to be measured.

Second, as the scattered wave is radiated in all directions, the scattered power returning to the ground is minute. By using a very powerful transmitter (several tens of kilowatts mean power) these problems can be overcome and with sensitive spaced receivers coupled to narrow-beam directional aerials the electron density as a function of height can be measured. If the Doppler shift of the reflected signal is also observed the line-of-sight velocity of the electrons can be found. Analysis of the spec-

trum of the returned pulse also gives the electron and ion temperature of the region.

Those who used this technique previously only operated one receiver and thus only measured a single component of the velocity at any one time. A complete picture is built up by observing different points of the ionosphere at different times. The big drawback of this method is that spatial and temporal variations can not be distinguished. Preliminary results of the first spatio-temporally coherent measurements of electron densities and velocities using the Thomson scattering method are set out on page 109 of this issue of *Nature*. Taylor, Rishbeth and Williams achieved this by using the Malvern radio transmitter and receivers at Aberystwyth, Jodrell Bank and Chilbolton; the receivers were each about 110 km away from the transmitter. The beam width of the receiver aerials was 2° , giving a height resolution of 13 km in the E region

and 40 km in the F region. Simultaneous measurements were made of the same scattering volume, this being vertically above Malvern. The electron velocity can be measured to an accuracy of $\pm 12 \text{ m s}^{-1}$, and by spectrally analysing the returned signal the electron and ion temperature at a specific height can be found to an accuracy of $\pm 15 \text{ K}$. These sensitivities compare favourably with the chemical trail observations but the great advantage of this incoherent scatter system is its relative cheapness and the fact that it can be run continuously.

Theorists will anxiously await more data from this group. A series of accurate observations of F layer densities, winds and temperatures and their variations with the time of day and the season of the year should provide the long-awaited key to an understanding of this mystifying region of the atmosphere.

D. W. H.

Recruitment of Cells in an Autoimmune Response

THE concept of tolerance originated from the observation by R. D. Owen (*Science*, **102**, 400; 1945) that in some dizygotic twins in cattle, erythrocytes of two antigenically different kinds could be found. The explanation, which emerged later from the work of Burnet and Fenner, was that during foetal life self recognition occurred after which a state of self tolerance pertained. In the cattle twins it was supposed that exchange of cells occurred as a result of placental fusion before the self recognition had taken place and that later the foreign cells became accepted and tolerated as self entities. Part of the early theory underlying this process was that self-reactive clones of cells were eliminated from the body. Tolerance on such a basis would involve the absence of any cells capable of reacting against a tolerated antigen. In recent years this general notion has been challenged. The communication by Cohen in next Wednesday's *Nature New Biology* (March 14) continues this questioning.

Earlier studies from the Weizmann Institute have shown that certain populations of rat lymphocytes, when grown on syngeneic fibroblast monolayers *in vitro*, can develop a cytotoxic capacity against the fibroblasts. This cytotoxicity can be blocked by the addition of syngeneic serum. Injection of lymphocytes sensitized on syngeneic fibroblasts into the foot pads of syngeneic recipients leads to enlargement of the draining lymph nodes. The interpretation of these

experiments is that there exist in normal rats cells capable of undergoing auto-sensitization but that this is usually blocked by serum factors. Once, however, the sensitization process has occurred then the activated cells can have a cytotoxic autoimmune effect. Cohen addresses himself in the latest series of experiments to the question of the source of the cells in the enlarged lymph nodes which follow injection of syngeneically sensitized lymphocytes into a syngeneic recipient. Irradiation of the donor cells after sensitization did not much reduce the lymph node swelling whereas irradiation of the host did. The cells in the lymph node were only markedly cytotoxic when swelling had occurred and Cohen is inclined to the notion that recruitment of autoimmune cytotoxic cells from the host is brought about by the injected donor cells.

It could be argued that the syngeneic fibroblasts *in vitro* have a neo-antigen because of their existence in culture, but it is difficult to see on this basis why the sensitized cells should react against an antigen *in vivo* which should not be there. Alternatively it might be thought that the sensitized cells were themselves antigenic and that the predominantly host response was against the grafted donor cells. Again it is difficult to see why the induced response of host cells should be able to operate against fibroblasts *in vitro*. Whatever the final resolution of these arguments the experiments are ingenious and deserve attention.

BIOMETRICS

Disease Inheritance

from a Correspondent

MULTIFACTORIAL models for the inheritance of liability to disease was the subject of a symposium held in London on February 15 and organized by the British Region of the International Biometric Society.

Some diseases are known to be caused by a single recessive gene. If this gene causes death before adulthood, then the parents of an affected child must both be heterozygous for the gene and hence the risk of the disease for any other child they bear is one in four. Many diseases seem to have a genetic component but not to be inherited in this simple way. Only a proportion of the individuals homozygous for the recessive gene may have the disease and some individuals not homozygous for the recessive gene may have it. The multifactorial model assumes that there are many genetic loci and many environmental factors involved in the causation of the disease.

Professor D. S. Falconer (University of Edinburgh) reviewed the development of the threshold model and his own work on diabetes. In the threshold model, each person has a liability to the disease that can be measured on some continuous scale. The individual succumbs to the disease if his liability exceeds some threshold value. The threshold may depend on sex and age. The liability is inherited in the same way as other continuous characters. The idea of an abrupt threshold may be biologically unacceptable but the threshold model is mathematically equivalent to a more realistic model in which some underlying variable takes different values for different individuals, correlated for relatives, and the probability of having the disease is a sigmoid function of this underlying variable. The parameters of the model are estimated from the incidence of the disease in the general population and the incidence of the disease in relatives of the affected individuals. Making some genetic assumptions, the model can be used to calculate risks for other types of relatives of the affected individuals.

Dr C. Smith (University of Edinburgh) discussed approximate and exact methods for computing risks when information is available for a number of relatives of an individual. Professor Falconer and Dr Smith discussed the evidence for a multifactorial determination of certain diseases. Dr Smith commented on the difficulties of distinguishing, on the basis of data on disease incidence, between models involving a single genetic locus and multifactorial models involving many loci.

Dr C. O. Carter (MRC Clinical

Genetics Unit, London) presented evidence, including some concerning sex differences in disease incidence, for the multifactorial determination of some common congenital malformations, such as harelip and cleft palate. He argued that the complex nature of foetal development and the prevalence of genetic polymorphisms support a polygenic model rather than a single gene model with low penetrance. The polygenic model gives a better fit to data on low frequency malformations. For higher frequency malformations the polygenic model is less easy to verify but the single locus model, together with the low fitness of parents, would imply a very high level of mutation.

Professor J. H. Edwards (University of Birmingham) argued that the multifactorial model may be used deductively to calculate recurrence risks but is of no use for the inductive development of more soundly based explanatory models. He saw future progress coming from an increased understanding of the basic but complex genetic determinants of the disease. He stressed, as did the other speakers, the dangers of always interpreting familial correlations as attributable to heredity rather than environment. In the discussion the possibility of virus transmission being a chief cause of familial correlations was mentioned.

The complexities arising from a range of patterns of causation even of a single disease were not discussed in detail. There was very little discussion of possible deficiencies in the data used to estimate disease incidence. The difficulty in discriminating between different models of inheritance has an advantage

in a certain robustness of the predictions from any model to the inadequacies of that model. The real questions concern the extent to which empirical data can be extrapolated to predict risks for more distant relatives and the extent to which more complex family data can be used to improve these predictions. When does the extrapolation give better estimates than available empirical data?

Dr Smith mentioned recent work on the use of values of a trait correlated with the underlying variables to improve the prediction of risk. This may increase understanding of the nature of the underlying variable. There can be no argument that statistical models are only a substitute for a more basic understanding. There will be different estimates of how long it will be before statistical models for disease inheritance are no longer necessary.

INSECT HORMONES

Ecdysone Problems

from our Insect Physiology Correspondent
ACCORDING to accepted theory the hormone responsible for initiating growth and moulting in the insect is secreted by the prothoracic glands when activated by the product of neurosecretory cells in the dorsum of the brain. The moulting hormone has been generally identified with the steroid ecdysone originally isolated by Butenandt and Karlson from developing pupae of *Bombyx*. But at present this theory is faced with certain problems. In the first place, the original ecdysone (α -ecdysone) is far less active in many insects than the more polar

A Ribosome's Muscle

ALTHOUGH the 50S and 30S subunits of *Escherichia coli* ribosomes have been reconstructed *in vitro* from appropriate mixtures of ribosomal proteins and RNAs, the function of most ribosomal proteins remains to be elucidated. Two exceptions are the L7 and L12 proteins of the 50S subunit which apparently function in translocation during protein synthesis. These two proteins are also remarkable because they differ in only one respect, the N terminal serine residue of protein L7 is acetylated whereas the N terminal serine of L12 has a free amino-group. In *Nature New Biology* next Wednesday (March 14) Wittman's group, Thammana *et al.*, report data which indicate that each 50S subunit contains at least two and probably three copies of L7 and/or L12.

Estimates of the amounts of L7 and L12 proteins that can be recovered from ribosomes labelled uniformly indicate that each ribosomal subunit

has at least one copy of each protein. A quantitative immuno-precipitation procedure also indicates that each 70S ribosome contains approximately three L7/L12 molecules. In short, all the data Thammana *et al.* have obtained indicate that in the ribosomes isolated from bacteria growing at different growth rates the total amount of L7/L12 remains constant—three molecules per ribosome—even though the proportion of acetylated L7 molecules to unacetylated L12 molecules may vary.

Why do ribosomes contain multiple copies of L7/L12 but only single copies of most other ribosomal proteins? Both L7/L12 have chemical similarities to contractile proteins which usually operate in multimeric assemblies. Thammana *et al.* suggest therefore that "the multiplicity of L7/L12 in the ribosome may reflect a requirement for a replete structure to affect movements of the ribosomal subunits with respect to each other".

hydroxy-derivative β -ecdysone, or ecdysterone. There is increasing evidence that ecdysone is converted by the insect into ecdysterone and that this is the active principle in moulting.

It has not proved possible, however, to extract either substance from the prothoracic glands of insects. Ecdysone can be extracted from adult *Bombyx*, in spite of the fact that the prothoracic glands break down completely before the adult emerges. In the eggs of *Bombyx* ecdysone reaches a high level before the prothoracic glands have developed (Ohnishi, Ohtaki and Fukuda, *Proc. Japan Acad.*, **47**, 413; 1971). Headless embryos of the locust *Schistocerca* undergo the two normal embryonic moults without the presence of the prothoracic gland (Miciarelli and Sbrenna). Moulting in *Rhodnius* can be prevented for several weeks if the haemocytes are put temporarily out of action by the injection of indian ink; but if ecdysone is injected there is no delay. In the caterpillar of *Calpododes*, if the head is tied off and a ligature applied between the thorax and the abdomen, the thoracic fragment will duly moult if ecdysone is injected into it; it will not moult if the prothoracic gland is implanted—unless a part of the abdomen is included in front of the ligature (Weir, *Nature*, **228**, 580; 1970).

The cytoplasmic changes that take place in the epidermis during wound healing are identical with those induced by ecdysone during moulting. These and other observations have led to the suspicion that the prothoracic glands do not secrete ecdysone (cf. Hoffmann and Joly, *CR Acad. Sci., Paris*, **257D**, 1665; 1972) but perhaps another hormone which induces ecdysone secretion by some other organ or tissue in the abdomen. Possible candidates are the haemocytes, fat body, pericardial cells, oenocytes, even the epidermal cells.

The other principal ecdysone problem is its site of action. Soon after the isolation of ecdysone in the early 1950s it was shown in *Rhodnius* that the earliest demonstrable effect of the hormone was the renewal of protein synthesis in those cells actively concerned in growth; as evidenced by nucleolar enlargement, a great increase in nuclear and cytoplasmic RNA within an hour or two of its application, coupled with rapid multiplication of mitochondria. This was the first rational interpretation of the mode of action of a growth hormone and was soon adopted also for the growth hormone of vertebrates. But the site of action has remained obscure.

The observation made originally by Clever and Karlson (1960) that ecdysone alters the puffing pattern in the giant chromosomes of larval Diptera suggested a site very close to the genes themselves. In the tanning of the integument in blowfly larvae, which is a very

sensitive test for ecdysone, the hormone induces the synthesis of the key enzyme DOPA-decarboxylase. Using inhibitors of RNA and protein synthesis, Sekeris (*Gen. Comp. Endocrinol.*, *Suppl.* **3**, 149; 1972) shows that synthesis of the enzyme protein is dependent on newly synthesized RNA. Ecdysone stimulates the synthesis of nuclear RNA within 1 h after administration and acrylamide gel electrophoresis, and appropriate labelling show that qualitative differences in cytoplasmic RNA are induced by the hormone action. Very recently Emmerich (*Gen. Comp. Endocrinol.*, **19**, 543; 1972) has demonstrated the existence of receptor proteins for ecdysone in the cytoplasm of *Drosophila* salivary glands. Complexes between protein and ecdysone have been recognized also in the nucleus, where they are confined to the non-histone fraction of the chromatin. Purified chromatin from the glands will also bind ecdysone on incubation *in vitro*, but to a lesser extent than occurs *in vivo*.

PATHOGENIC BACTERIA

Phyto and Entomo

from our Soviet Correspondent

EXPERIMENTS carried out at the Institute of Microbiology and Virology of the Ukrainian Academy of Sciences indicate that certain bacteria can be pathogenic for both plants and insects (*Dopov. Akad. Nauk Ukr. RSR*, Ser. B (1), 80; 1973). Unlike previous studies which postulated a symbiotic or parasitic relationship between phytopathogenic bacteria and insect "hosts", this investigation demonstrates that the same strain of bacteria can be both phyto- and entomopathogenic.

Various strains of the bacterial genus *Erwinia* were isolated from infected beech, cucumber and cauliflower plants. Cabbage leaves were treated with a suspension of the bacteria of strength 1×10^5 ml.⁻¹ and the leaves were fed to Colorado beetle larvae.

On the fifteenth day following infec-

Selection for RNA Secondary Structure

ALTHOUGH the nucleotide sequences of only a few messenger RNAs have been analysed, it is quite clear that many messenger RNA molecules contain extensive regions of base-paired secondary structure. Such secondary structure may have a role in controlling the transcription and/or translation of the messengers. Because the presence of base-paired regions in a messenger RNA restricts the number of different amino-acids that can be coded by that messenger, it can be argued that during the evolution of some proteins, selection pressure has acted not only directly on the amino-acid sequence of the proteins but also on the RNA molecules themselves, so as to maintain some necessary secondary structure.

Is there any evidence for direct selection, during evolution, for messenger RNAs with particular secondary structures? Ball, who in *Nature New Biology* next Wednesday (March 14) reports an interesting analysis of the codons of the RNA bacteriophage MS2 coat protein cistron, believes that "the amino-acid sequence of MS2 coat protein has been subjected during its evolution to rearrangement in the interests of the secondary structure of the messenger RNA".

As Ball points out, there seem to be three possibilities controlling the relationship between an amino-acid sequence and the secondary structure of the messenger RNA that specifies that amino-acid sequence. First, selection pressure might act solely on the amino-acid sequence without regard to the secondary structure of the RNA. Second, selection might act not only

on the amino-acid sequence but also on the choice between synonymous degenerate codons so as to maximize secondary structure of the RNA within the limits set by the amino-acid sequence. Third, selection pressure might act on the choice of synonymous and non-synonymous codons changing the amino-acid sequence in the interests of the secondary structure of the messenger RNA.

The great extent of base pairing of MS2 coat protein RNA seems to eliminate the first possibility. By classifying codons according to the number of base options (alanine codon $\begin{smallmatrix} U \\ GC \\ A \\ G \end{smallmatrix}$ is classified as having base

options 1,1,4) Ball has analysed the codons that occur in the base paired regions of MS2 co-protein RNA. He concludes that "there is no clear correlation between the degree of pairing and the number of base options indicating that the degenerate positions do not make an exceptional quantitative contribution to pairing".

If this is the case, as it seems to be, then one can only conclude that selection pressure has acted directly on the sequence and therefore secondary structure of this particular mRNA. In other words, the amino-acid sequence may have evolved in the interests of the secondary structure of the RNA molecule. Whether such selection is widespread remains to be seen, for it always has to be remembered that MS2 RNA has to be encapsidated in a virus particle and may therefore have been subjected to selection pressures not exerted on cellular messengers.

tion, mortality among the larvae infected with certain strains of phytopathogenic bacteria was significantly higher than among the control larvae which had been fed non-infected leaves. The highest mortality rates were observed for *Erwinia* sp. 418₃ and 449₂ isolated from beech and for *E. ardoideae* 144 isolated from cauliflower (53.3 ± 6.7 per cent, 58.5 ± 1.6 per cent and 40.2 ± 2.6 per cent respectively, as against 20.4 ± 11.3 per cent among the control larvae). By day 20, mortality from *Erwinia* sp. 418₃ was 93.3 ± 6.7 per cent as against a control value of 23.8 ± 11.1 per cent. Significant increases were also observed for *Erwinia* sp. 262_{2a} and 521_{3a} from beech (81.3 ± 6.3 per cent and 74.6 ± 5.1 per cent respectively after 26 days, control value 46.6 ± 9.7 per cent). Other strains of *Erwinia* from beech and cauliflower and strains obtained from cucumbers produced no statistically-significant increase in mortality.

After 26 days imago formation took place among the surviving larvae, and further development proceeded normally. The number of eggs laid by the adult beetles varied considerably but seemed unrelated to the type of infection. The authors conclude that the entomopathogenic properties of phytopathogenic bacteria depend on the bacterial strain and its natural habitat. It is noteworthy that anaerobic strains in general had a greater effect on the larvae, irrespective of their origin.

In a converse series of experiments, entomopathogenic bacteria were isolated from the haemolymph of sugar-beet weevil larvae, and identified as being of types close to the phytopathogenic *E. caratovora* and *E. ardoideae*. These were found to produce disease in onion seedlings and unripe tomato fruits, with symptoms similar to those from typical phytopathogenic strains. The *E. ardoideae* analogue seemed more virulent than that of *E. caratovora*.

It seems therefore that in natural conditions certain plants and insects may be susceptible to the same pathogenic bacteria — a fact which, the researchers stress, should be taken into account in any attempt to develop microbiological methods of combating crop pests and diseases.

BREAST CANCER

Anti-oestrogen Therapy

from a Correspondent

ADMINISTRATION of sex hormones or their analogues, whether androgenic, oestrogenic or progestogenic, can produce temporary remission in about one-quarter to one-third of patients with advanced breast cancer. Preparations of anti-oestrogens have also been used, for example, ethamoxitriethetol ('MER-25'), triparanol and clomiphene citrate,

three compounds which are analogues of the non-steroidal oestrogen chlorotrianisene (chlorotris (p-methoxyphenyl) ethylene). The number of patients treated by these compounds, however, has been too small for any conclusion to be drawn about their usefulness.

There have, however, been trials recently in women with advanced breast cancer of a new anti-oestrogen, 'Tamoxifen' (ICI 46,474), which is related chemically to clomiphene. Cole *et al.* (*Brit. J. Cancer*, **25**, 270; 1971) used doses of 10 to 20 mg daily in patients who had already been treated by a variety of means. Ten of the forty-six patients treated had a good response as judged by reduction in size of the soft tissue masses and/or radiological evidence of regression of pulmonary or bone metastases and a further seventeen patients had a partial response. The response rate was similar to that seen with other forms of hormone treatment. Side-effects were mild; only two patients stopped treatment because of them.

Better results have now been reported by Ward (*Brit. Med. J.*, **1**, 13; 1973) in a study of sixty-eight patients with advanced breast cancer. Sixty per cent of the patients receiving daily doses of 20 mg of 'Tamoxifen' and 51 per cent of those receiving 10 mg each day achieved some reduction in the size of the tumour and in 38 per cent of the women this reduction was to one-half or less. Breast tissue and breast tumours contain "receptor" proteins which take up and bind oestrogens and it is possible that 'Tamoxifen' acts by preventing the uptake of oestrogens.

Double Helix from Mononucleotides and Polymer

It has not so far been evident that double-helical structures are formed when mononucleotides associate with a single-stranded polynucleotide. For the system of adenine with polyuridylic acid the evidence is that only triple-stranded helices, with a base ratio of 2U:1A, are formed. Now Pörschke, Hoffman and Senear, writing in next Wednesday's *Nature New Biology* (March 14), have found that a two-stranded complex, just like poly (A) · poly (U), can in fact be formed when an adenine derivative stacks onto poly (U). In N-6,9-dimethyladenine the additional hydrogen bonding site required for the attachment of a second poly (U) strand is blocked. The dimethyladenine also has a strong stacking propensity, and binds readily to poly (U).

Miles and his associates have already shown that the infrared spectrum of the complex is similar to that of poly (A) · poly (U). The first demonstration that Pörschke *et al.* give of 1:1 stoichiometry is by equilibrium dialysis, in

VIRUSES

The RSV Genome

from our Cell Biology Correspondent

Rous sarcoma virus was discovered in 1911 by Peyton Rous and if anybody could raise the energy to compile a complete bibliography of reports of work involving this virus they would surely end up with a massive tome which might well continue growing exponentially for some time to come. But in spite of the vast effort that has been spent and the vast literature that has accumulated the size and structure of the RNA genome in RSV particles are still not known for certain.

The problem is that none of the RNA molecules that can be extracted from the virions is reproducibly infectious, so although the 60-70S RNA in these particles probably is or includes the viral genome this has never been proven unequivocally. This 60-70S RNA (and let us assume it includes the viral genome because the properties of the 4S, 7S and other minor RNAs that can be extracted from RSV particles strongly suggest that they cannot be genomic) has curious properties. Most notably on denaturation, either by heating or treatment with denaturants such as DMSO, it sediments at 30-35S instead of 60-70S. Moreover, the 30-35S RNA has a greater electrophoretic mobility than has the 60-70S RNA. Clearly these differences can be explained in two ways; either the 60-70S RNA is a single polynucleotide chain which can undergo dramatic

which the binding ratio is incompatible with saturation at one dimethyladenine to two uracil residues. Second, a spectrophotometric continuous-variation mixing experiment gives a sharp maximum of hypochromicity at a base ratio of 1:1. The binding is cooperative and the complex displays a sharp melting curve like a two-stranded polymer.

That base pairing with hydrogen bonding is indeed involved was confirmed by showing that 6-N,N'-dimethyladenine, in which both hydrogen atoms at position 6 are replaced, does not form a complex with the poly (U). By the temperature jump method, the kinetics of the interaction of the monomer with poly (U) were found to be characterized by a relaxation time in the μ s range, which is about three orders of magnitude slower than triple-strand interactions, in which there are three reactants. The stacking of monomers alone, in the absence of base pairing, occurs at a rate which is some three orders of magnitude faster again.

changes in conformation, or the 60-70S RNA comprises an assembly of 30-35S subunits held together by heat labile, DMSO labile bonds—in short, hydrogen bonds. If the second model is correct, as is widely believed, and if relationships between sedimentation coefficients and molecular weight derived for small RNA molecules are valid it is simple to calculate that the 60-70S RNA has a molecular weight about $10-12 \times 10^6$ and that the putative 30-35S RNA subunits have a molecular weight of about $3.3-3.5 \times 10^6$.

Two lines of evidence support the notion that the 60-70S RNA of RSV is an aggregate of subunits—in other words that the RSV genome is segmented; first, when chick fibroblasts are infected by two different strains of RSV recombinant virus particles form a considerable proportion of the progeny. This high frequency of recombination, which is also a characteristic of influenza virus—a virus with a single segmented RNA genome—can readily be explained if the RSV genome comprises three or four RNA chains, for in mixed infections progeny virions may assemble by taking combinations of RNA subunits of the two parental types. Second, Cheung *et al.* (1972) and Canaani *et al.* (1973) have just reported quite different physico-chemical evidence which suggests that 30-35S RNA may be a precursor of 60-70S RNA.

Cheung *et al.* (*Virology*, **58**, 51; 1972) harvested Prague strain RSV from chick fibroblasts at 5, 10, 20, 60 and 180 min and 12 h and 24 h intervals. The 5 min harvest virus proved to contain little 68S RNA, but it contained a heterogenous RNA with a median sedimentation coefficient of 55-60S which on denaturation yielded 36S RNA and RNA sedimenting between 36S and 4S. By contrast 24 h harvest virus particles yielded a homogenous 68S RNA which denatured to yield homogenous 36S and 4S RNAs. Particles harvested at intermediate times yielded RNA patterns which were intermediate between these two extremes.

Canaani *et al.* (*Proc. US Nat. Acad. Sci.*, **70**, 401; 1973), who found themselves down wind, so to speak, of Cheung and his colleagues, made similar experiments harvesting Prague RSV particles at only 3 min intervals. They then compared the sedimentation properties and electrophoretic mobilities of the RNA in 3 min harvest virus with the RNAs of virus harvested at hourly intervals. They found the rapidly harvested virus contains a little 60-70S RNA but a large amount of 30-4S RNA and a variable amount of 4-12S RNA. Furthermore, incubation at 40° C for 3 min of the 3 min harvest virus results in the conversion of most

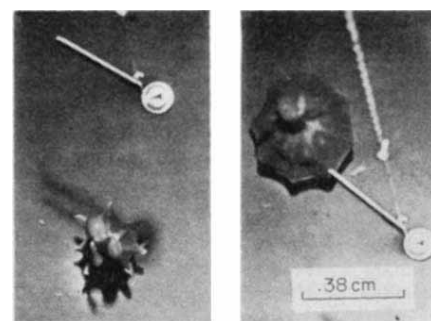
of the 30-40S RNA into 60-70S RNA.

These results suggest that 30-4S RNA is a precursor to 60-70S RNA and, as Canaani *et al.* point out, the conversion may involve the association of 4S RNA. They say this for two reasons; first, when 60-70S RNA is denatured back to 30-40S RNA five times more 4S RNA is released than is released when 30-40S RNA from 3 min virus is denatured directly; second, 30-40S RNA from 3 min virus is about five-fold less efficient as a template for reverse transcriptase than 60-70S RNA, and 4S RNA is believed to act as a primer for reverse transcription. Of course, the report (Jarrett *et al.*, 1971) that some free 30-40S RNA can be isolated directly from feline leukaemia virus particles harvested at long intervals gains significance in the light of these experiments.

CEPHALOPODA

Deep-sea Cirromorphs

DEEP-SEA cirromorphs (Cephalopoda) have been recorded and photographed for the first time in the Arctic Ocean (Pearcy and Beal, *Deep-sea Res.*, **20**, 107; 1973). Out of more than 2,900



photographs taken off Point Barrow, Alaska, from USS Staten Island, cirromorphs appeared in twenty-one, representing a maximum of twelve individuals; all were at depths ranging from 3,219 to 3,786 m.

The cirromorph in these photographs (taken 15 s apart) is swimming upright or perpendicular to the bottom, with its fins appearing to stroke horizontally. The arms and web in this sequence are first joined together and extend toward the bottom (left) and then expand laterally to form a large umbrella-like surface (right). A second mode of locomotion—swimming in the horizontal position, which is more typical of cephalopods—was also noted.

Initiation of Eukaryotic Protein Synthesis

NEXT week, in *Nature New Biology* (March 14), Schreier and Staehelin present a detailed study of the roles of two purified protein factors involved in polypeptide chain initiation in eukaryotes. These authors suggest that the mRNA-independent binding of the initiator Met-RNA_i to the 40S subunit is the first step in polypeptide chain initiation in eukaryotes and that this complex then directs the building of mRNA.

Four initiation factors, IFE_{1,2,3,4}, were purified from rabbit reticulocytes by means of DEAE-cellulose followed by preparative glycerol density gradients. IFE₂, IFE₃ and GTP are absolutely required for the binding of Met-RNA_i to 40S subunits in the absence of mRNA. Two forms of IFE₃ with sedimentation coefficients of 15S and 17S were detected, the smaller form probably being a degradative product of the larger.

With initiator Met-RNA_i purified IFE₂ but not IFE₃ formed a complex which was GTP-dependent but ribosome and template-independent. In the presence of artificial template poly (A,U,G) and GTP, IFE₃ alone did not promote formation of the [40S Met-tRNA_i] initiation complex, whereas IFE₂ did. When natural globin mRNA replaced the artificial template, IFE₂ no longer promoted binding of Met-tRNA_i and 40S subunits, but on the addition of IFE₃ to IFE₂ the complex was formed. Thus

IFE₃ is required for binding of natural mRNA. Complex formation in the presence of IFE₂ and IFE₃ was just as efficient when mRNA was omitted. Thus IFE₃ promotes template-independent binding of Met-tRNA_i (presumably complexed with IFE₂ and GTP) to 40S subunits.

The [Met-tRNA_i 40S] complex sedimented very close to the original 40S subunits. But in the presence of excess IFE₃, subunits which had not bound Met-tRNA_i sedimented at about 48S, a sedimentation rate compatible with a particle composed of a 40S subunit complexed with IFE₃. Schreier and Staehelin suggest that IFE₃ combines initially with a 40S subunit to direct the binding of Met-tRNA_i. Subsequently the bulk of IFE₃ must dissociate from the initiation complex but a component of it may remain attached, to direct binding of mRNA. If IFE₃ were heterogeneous with regard to such a component, this might provide a mechanism for mRNA selection.

In conclusion, IFE₃ promotes the mRNA-independent binding of Met-tRNA_i to the 40S ribosome subunit in the presence of IFE₂. In that the initiator tRNA is bound first to the ribosome and helps the binding and correct phasing of messenger rather than the converse, this model of polypeptide chain initiation in eukaryotes is fundamentally different from the bacterial mechanism.

MUSCLE

Regulating the Scallop

from our Molecular Biology Correspondent

ONE of the most interesting developments in muscle biochemistry in recent years is the partial resolution of the regulatory machinery, which enables the myosin ATPase to respond to small changes in the concentration of calcium supplied by the sarcoplasmic reticulum. In the mammalian systems, on which most of the work until just now has been done, the site of regulation is in the thin filaments, the effect of the calcium, which binds to one of the troponin subunits, being transmitted by way of the other troponin components and tropomyosin to the actin. This causes a change in the affinity of the actin for the myosin heads, reaching out towards it from the thick filaments. Szent-Györgyi and his colleagues have been looking at the muscles of more primitive creatures, and here it seems that regulation is encompassed in a totally different manner, and operates not at the thin filaments but rather the myosin heads. Szent-Györgyi, Szentkirályi and Kendrick-Jones (*J. Mol. Biol.*, **74**, 179; 1973) now unfold a remarkable story concerning the mechanism of regulation of scallop muscle.

Fortune has to an extent favoured them, for the wily scallop has incorporated a regulatory subunit into its myosin, which, unlike the minor chains of numerous other myosins that have been looked at, can be removed without reducing the protein to an enzymatically inert porridge. The myosin extracted from scallop muscle is superficially indistinguishable from that of mammals: its hydrodynamic and optical rotatory properties are the same, and so is its length in the electron microscope, as well as the segment long spacing in fibres. When exposed to proteases under the right conditions it gives rise to enzymatically active soluble fragments, corresponding to heavy meromyosin and subfragment-1 (or isolated heads). Also like rabbit myosin, it contains light chains, which in SDS-gel electrophoresis come out at a molecular weight of about 18,000, and in a ratio of three chains per myosin molecule.

There are, it turns out, two types of light chain in the scallop, one of which contains cysteine and is present in a ratio of two copies per myosin. The other, of which there is only one copy per myosin molecule, is dissociated when EDTA is added to sequester divalent metal ions. It is this molecule which is the "regulatory subunit", in that it confers calcium response on the myosin. Thus, the ATPase activity is unaffected by the addition of calcium ions when the critical light chain has been stripped off, whereas the ATPase of intact myosin

with its complement of light chains is strongly inhibited by removal of calcium. When the preparation of light chains is added back to the stripped myosin, the calcium sensitivity is restored. The stripped myosin still binds calcium, but binding plots indicate that probably one site has been lost. The light chain is not of itself, however, capable of taking up calcium. The other (cysteine-containing) light chains remain firmly stuck to the myosin, and their removal by more drastic methods leads to irreversible denaturation.

The independence of the thick and thin filament-dependent regulation mechanisms is spectacularly demonstrated by the interaction of the desensitized scallop myosin, denuded of its regulatory light chain, with the rabbit actin-tropomyosin-troponin system, with which it combines just like rabbit myosin to give a fully calcium-regulated actomyosin. This points again to the evolutionary stability of actin compared with myosin, at which evolutionary pressures are evidently mainly directed. Rabbit myosin will induce a high and calcium-independent ATPase activity in a mixture of actin with excess scallop myosin. It seems therefore that when there are no calcium ions about, the scallop myosin does not combine with actin, so that the actin filaments are available for reaction with a competing species. Neither, in consequence, is the ATPase of the scallop myosin stimulated by actin in the absence of calcium. Like the regulatory protein system of mammalian thin filaments, then, the sensitizing subunit of the scallop myosin blocks the interaction with actin. The oddest aspect of this engrossing story is the stoichiometry. In principle it is possible, with the aid of some allosteric hand-waving, to explain the calcium response of a system bearing a regulatory subunit on only one myosin head. The authors eschew such intellectual contor-

tions, and postulate instead that the regulatory chain physically links the two heads, and functions only when so located. In support of this kind of scheme is the finding that active isolated heads (8-1) from the scallop myosin contain the regulatory light chain, but have no calcium sensitivity.

A curious twist to the picture of regulation in mammalian muscle emerges from an observation by Bailin and Bárány (*J. Biol. Chem.*, **248**, 373; 1973), who find that rabbit myosin, dinitrophenylated to the extent of 1.5 modifying groups per molecule, displays no calcium sensitivity in its complexes with actin, tropomyosin and troponin. Partial removal of the dinitrophenyl groups leads to reappearance of calcium sensitivity. This seems to indicate that the myosin when modified no longer recognizes the difference between the inhibiting and non-inhibiting states of the thin filaments.

SOLAR SYSTEM

Primordial Field

from our Geomagnetism Correspondent

THE strength of the interplanetary magnetic field is now of the order of tens of gammas. But has it always been so? And in particular, was it quite so low at the time of the origin of the Solar System? In the absence of any other information, it is often taken as axiomatic that the interplanetary field has always been negligible; but Sonnet *et al.* (*Astrophys. Space Sci.*, **7**, 446; 1970) have postulated otherwise, and primordial magnetic fields of the order of 1 oersted ($10^3\gamma$) are apparently required by the cosmological theories of Fowler *et al.* (*Geophys. J.*, **6**, 148; 1962).

In principle, it should be possible to determine the intensity of the primordial interplanetary field from carbonaceous chondritic meteorites, for Banerjee and

Drag Reduction for a Rotating Disk

ALTHOUGH drag reduction in turbulently flowing liquids containing small amounts of polymers like polyacrylamide is fairly well investigated, both practically and theoretically, it is the possibility of drag reduction under conditions of laminar flow that is in many ways of particular interest. For one thing laminar flow is more commonly encountered than turbulent flow.

In next Monday's *Nature Physical Science* (March 12) Kale, Mashelkar and Ulbrecht show that drag can be reduced when a disk rotates under laminar conditions in an appropriate polymer solution. What they have done is to investigate the relationship between torque and angular velocity for disks between 7.5 and 15 cm in diameter in solutions

of carboxy methyl cellulose—an inelastic fluid—and in three solutions of polyacrylamide of varying strength. It turns out that for a given angular velocity (plotted by Kale *et al.* as a moment coefficient) the torque is significantly reduced in the case of the polyacrylamide solutions.

Kale and his colleagues draw attention to the fact that others have shown drag reduction in laminar flow to be possible in situations like flow in a curved tube and flow round a sphere, but they place their own work firmly on a practical footing when they say that "for centrifugal pumps with enclosed disk-type impellers there is likely to be a better pumpability with viscoelastic liquids".

Hargraves (*Earth Planet. Sci. Lett.*, **10**, 392; 1971) recently showed that some such chondrites contain a moderately strong and stable natural remanent magnetization (NRM). In this branch of geophysics, of course, the gap between principle and practice can be quite wide, as the difficulties in obtaining palaeointensities from even very young terrestrial rocks prove. Nevertheless, both Banerjee and Hargraves (*Earth Planet. Sci. Lett.*, **17**, 110; 1972) and Butler (*Earth Planet. Sci. Lett.*, **17**, 120; 1972) have independently succeeded in determining at least minimum intensities of the primordial field and find them to be several orders of magnitude higher than commonly supposed.

Banerjee and Hargraves have used the Orgueil, Murchison and Allende meteorites which are carbonaceous chondrites representative of Wiik's (*Geochim. Cosmochim. Acta*, **9**, 279; 1956) Types I, II and III respectively. The principal field determination was carried out using a modified form of the Thellier method (backed up by thermomagnetic, opaque mineralogy and electron probe microanalysis studies) to assess stability and identify the magnetic carriers. The Allende chondrites proved to be the most stable magnetically (thus giving the most reliable data), the magnetic carriers being very fine single-domain grains, probably of NiFe. Even so, only temperatures up to 135° C could be used for the intensity determination because of thermally-induced chemical changes at higher temperature. The result of four determinations on two samples was an average palaeointensity of 1.09 oersted. The Murchison and Orgueil chondrites were relatively less stable, the usable temperature ranges being lower. Average palaeointensity values from these were 0.18 oersted and 0.67 oersted, respectively.

Butler has concentrated solely on the Allende meteorite and finds an average ancient field strength of 1.1 oersted. In a situation fraught with so many practical problems, such agreement is startling and, as Banerjee and Hargraves admit, must be regarded as fortuitous. The order of magnitude agreement is, however, important. Moreover, there are other reasons for regarding exact values with some caution. For example, the intensity determination methods used are based on the assumption that the NRM of the samples is TRM (thermoremanent magnetization). In the case of the Allende chondrite there is convincing evidence that this assumption is warranted, although in the other examples the validity is less secure. The point to bear in mind here is that TRM is the most effective magnetization process, so that if other processes were responsible for the magnetization the obtained palaeointensities would be underestimates. In other words, the

values derived by Banerjee and Hargraves and Butler are at least minimum values.

Because there is no evidence to suggest that the Allende chondrite has been reheated since its formation (indeed, there is evidence to the contrary), the conclusion must be that magnetization took place in a field at least 10^4 times higher than the present interplanetary field and of the order of magnitude of the present terrestrial surface field. This presumably refers to 4.44×10^9 yr ago, the potassium-argon age of the chondrules in the Allende meteorite. As Butler points out, however, alternative interpretations are possible. The strength of about 1 oersted only applies to the ancient interplanetary field if the Allende was actually magnetized in that field. It could, of course, have acquired its magnetization in the field of its parent body. In this case, the parent planet would have required an extremely high magnetic moment at a very early stage in the formation of the Solar System.

SEMICONDUCTORS

What is New?

from a Correspondent

THE opportunity for asking this question was afforded by the celebrations in connexion with the twenty-fifth anniversary of the transistor and, in particular, in a symposium held by the Institution of Electrical Engineers in London on February 14. A small exhibition of the

development of the transistor is open at IEE headquarters and is worthy of a visit.

The symposium did not forget the social aspects of the invention of the transistor, which were reviewed by Professor W. E. Farvis (University of Edinburgh); but the technical contributions and the lively discussion which followed concentrated rather on the question of the next step in the field of electronic devices. The five invited speakers were all members of research laboratories attached to leading British semiconductor manufacturers and they were all clearly adept at presenting the potential profitability of their ideas for new developments to their financial managers. They thus had an interest in presenting at least an appearance of hard-headedness. Although this rather limited the distance of the look forward and inhibited the range of an otherwise lively discussion, it allowed a firm and highly informal contemplation of the next stages in silicon technology.

The first talk was perhaps an exception to this generalization. Dr J. Evans (Standard Telecommunications Laboratories) talked about new materials and the new electronic functions which they might perform in the future. He began with an account of the versatility of gallium arsenide, which was at first used as a material for bipolar transistors but has now unexpectedly found application in light-emitting devices and as a microwave oscillator and a field-effect transistor. He held this up as an example of how to pick new materials

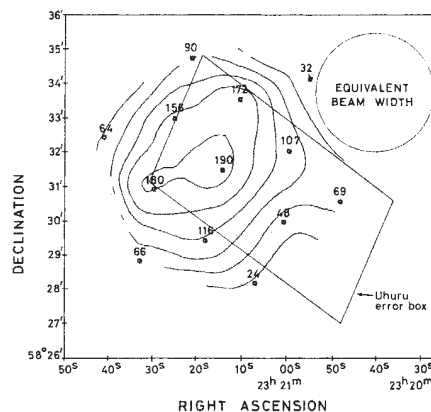
Soft X-ray Structure of Cassiopeia A

OBSERVATIONS of Cas A from Copernicus are reported in next Monday's *Nature Physical Science* (March 12) by Fabian, Zarnecki and Culhane. The diagram shows an X-ray contour map of the source in the energy range 1.4 to 4.2 keV, superimposed on the Uhuru error box for the source 2U2321 + 59. Clearly, the two objects are the same, as was already suspected.

The detail shown in the contour map is reminiscent of the first radio contour maps obtained of this source, and the overall distribution of X-rays is best explained by an annular source of outer diameter 5.5 ± 1.5 arc min and inner diameter 2.0 ± 2.0 arc min—this is definitely not a compact source of X-rays, even though it might have been expected that a collapsed object would have been left at the centre of this supernova remnant, which is only about 300 years old. "The data are suggestive", say Fabian *et al.*, "of a shell source" in which X-ray generation occurs either through thermal bremsstrahlung from a blast wave or through the synchrotron process.

Much remains to be discovered from

further detailed studies of the source; it is particularly interesting to compare the present X-ray observations with early radio maps of Cas A and with the best detailed map now available, made at 2.7 GHz, which the MSSL team reproduce in their article. If the accuracy of X-ray astronomy proceeds at the present pace, it may not be long before comparable high resolution X-ray maps of Cas A and other extended objects are available.



for research. Choose one with versatility, he said, because, even if it does not satisfy the original requirement, several other unexpected uses are likely to emerge. On this principle, STL has just announced an extensive commitment to research in glasses, which may be useful as detectors, switches, memories, light guides and in about twenty other ways listed by the speaker. Dr Evans listed several electronic functions (lasers, surface acoustic wave transducers and so on) which should find important new applications in the future.

Dr D. H. Roberts (Plessey, Allen Clark Research Centre) confined his attention to the "third generation" of transistor devices—silicon bipolar integrated circuits. He left out metal-oxide-semiconductor (MOS) integrated circuits on the grounds that they are difficult to control—although this implied deficiency did not go undefended in the discussions. He examined the new techniques of making arrays of very small transistors and envisaged a single bipolar device smaller than $1,000 \mu\text{m}^2$. The first commercial transistors were more than 1,000 times larger. Dr Roberts rightly termed knowledge of the physics of diffusion and other silicon processing techniques as "pathetic", and appealed for more work in that field.

Two subsequent speakers performed similar tasks of presenting the problems and plans for packing more logic functions into a single slice of silicon (an art called "large-scale integration"). One wants to get 5,000 logic gates on a silicon chip with dimensions no larger than 1 cm. Mr R. A. Hilbourne (Mullard, Southampton) dealt chiefly with computational logic circuits and Mr W. Holt (Plessey, Allen Clark Research Centre) dealt with semiconductor memories. Mr Hilbourne stressed that circuit design techniques, rather than new material technology, hold the key to this art but thinks that it is difficult to decide who should do the design—the user or the chip manufacturer. Hewlett-Packard, for example, left the detailed chip layout for their pocket calculator in the hands of the chip supplier but carried out a parallel computer simulation study of their own. The aid of the computer in layout and testing is absolutely essential. Even with completely computerized testing, Mr Hilbourne was worried about the "escape limit"—the areas of integrated chip which, for reasons of time, could not be tested, even by an ultra-fast computer checkout, before the chip was packaged and sent to the customer. Mr Holt's account was principally of the internal battle waged between various semiconductor approaches and the external battle between semiconductors and other memory techniques such as magnetic cores, magnetic bubble

domains, plated wires, tapes and cards. The market is great, the memory requirements are varied and so are the technological approaches. This leads to the "memory-of-the-month-effect", he said, a confusion and flux of fortunes which must be a manufacturer's nightmare. Nonetheless, Mr Holt guided the symposium through the relative merits of bipolar transistors, MOS in several forms and metal-nitride-oxide-semiconductor (MNOS) devices with clarity. One can, however, expect 10^8 years of stable data storage, even from the volatile-looking MNOS system.

Dr D. Burt (GEC, Hirst Research Centre) closed the technical session with an account of the work of the centre on the charge-coupled device (CCD)—an offshoot of MOS technology. His clear account of the principles made it easy to follow the various approaches GEC has made to dynamic storage of information and to imaging. GEC expected trouble in producing gaps between electrodes of $2 \mu\text{m}$ for great total elec-

trode lengths (100 cm on a large chip). Trouble was not encountered in the laboratory but Dr Burt wondered how this process would fare in production. The trouble with the device principle is that one is juggling with data bits represented by a few pC of charge (only 10 million electrons). Surface states could suck these up and delay the necessary crisp transfer along the chain of "potential wells" in the semiconductor. GEC is managing transfer efficiencies of 99.5 per cent and Bell Telephone Laboratories claims 99.8 per cent.

Perhaps here, in the tail of the symposium, was the most forcible riposte to the earlier statements that process control in the MOS device system is too difficult for its wide competitive success over Shockley's bipolar transistor in the future. Not only may the MOS logic circuit make inroads into the bipolar market but its offshoot, the CCD array, may also replace television camera tubes in due course.

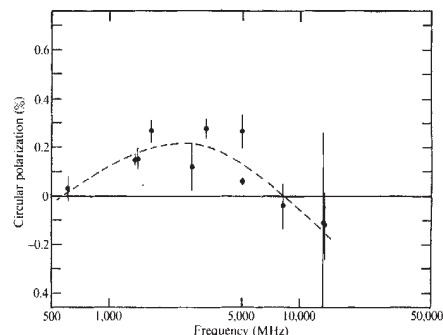
Circular Polarization in Compact Radio Sources

THE first observational investigation of the frequency dependence of circular polarization of compact radio sources is reported in next Monday's *Nature Physical Science* (March 12). Although the existence of such polarization in some sources has been known for two or three years, the measurement of the very low levels of polarization requires very careful measurements. Simultaneous observations to determine frequency dependence of the polarization require, in addition, collaboration by teams operating different antennas at widely separated points on the Earth. So these observations are something of a landmark in the history of radio astronomy.

The antennas used were the 140-foot at the National Radio Astronomy Observatory (NRAO), the NRAO interferometer and the 150-foot at Algonquin (ARO). These provided simultaneous measurements at 1,666 MHz, 2,695 MHz, 8,085 MHz and 13,500 MHz. In addition, some data obtained at different times with the ARO instrument may usefully be incorporated in the study, which was undertaken by Seaquist, Gregory, Biraud and Clarke. The three sources studied are 3C 138, 3C 279 and BL Lac. Each of them is circularly polarized at 1,666 MHz, but only 3C 138 shows marginal evidence for such polarization, at the 2 sigma level, at the higher frequencies.

By utilizing data obtained at other epochs, Seaquist *et al.* conclude that the degree of circular polarization for 3C 279 is small at both low and high frequencies and large at intermediate frequencies (see diagram), with a maximum

around 3,000 MHz. This suggests that one of the four synchrotron-emitting components believed to make up this source is much more strongly polarized than the other three. 3C 138 also shows evidence of variation of degree of circular polarization with frequency.



But perhaps most interest in this work will be focused on BL Lac. The measurements provide a new test by which the properties of this object can be compared with those of QSOs; unfortunately BL Lac is rapidly variable in circular polarization, which makes it useless to combine data from different epochs. Taken alone the November 1972 data indicate polarization at 1,666 MHz and hint at polarization at 2,695 MHz. But even these observations must be regarded as dubious now that BL Lac has been found to vary strongly over timescales of a few hours (Weistrop, *Nature Physical Science*, **241**, 157; 1973; see also *Nature*, **241**, 502; 1973). Really useful information about the circular polarization of BL Lac must, it seems, await another quantum jump in observing technique.

The Loch of Strathbeg

W. R. P. BOURNE

Department of Zoology, University of Aberdeen

C. H. GIMINGHAM

Department of Botany, University of Aberdeen

N. C. MORGAN & R. H. BRITTON

The Nature Conservancy, 12 Hope Terrace, Edinburgh

The British Gas Corporation and Total Oil Marine have applied for outline planning permission to build a reception and transmission plant for North Sea gas on the disused airfield at Crimond in Aberdeenshire. This could have a considerable effect on the Loch of Strathbeg, Britain's largest coastal dune lake, which lies alongside the airfield, particularly because the proposed access for pipelines crosses both loch and dunes. This article describes why the loch is of particular interest.

THE Loch of Strathbeg in north-east Aberdeenshire, the largest coastal dune lake in Britain, is a lagoon approximately 3 km long, a little less than 1 km wide, 220 ha in area and 1 to 2 m deep, which was formed when a sand bar blocked the entrance to the ancient port of Rattray at the beginning of the eighteenth century¹⁻³. Its water level fluctuated markedly at first, but was stabilized when a new outlet was cut in an unsuccessful attempt at drainage at the start of the period of agricultural "improvement" when the surrounding estates were put in order and planted with trees at the end of that century. The whole area has remained comparatively undisturbed since that time, apart from temporary use of the loch as a base for seaplanes during the First World War, and the establishment of an airfield at Crimond overlooking it from the west during the Second World War, when it was filled with moored logs to prevent hostile seaplanes from landing. Because the loch is now the best representative of its kind in Britain in terms of extent and freedom from alteration and pollution, is an important staging post for migratory waterfowl, and lies in the centre of an area which now seems likely to become increasingly industrialized, it seems timely to summarize the information available about it.

Geomorphology

The evolution of the area has been studied by Walton⁴. The underlying rock is gneiss, with occasional granite and quartzite outcrops, and slopes gently to the east. It is overlain by glacial deposits inland, but appears as a series of low headlands projecting through the dunes along the coast. It seems that during the last glaciation this corner of Aberdeenshire may have escaped the ice, and a series of indistinct raised shorelines inland may date from the last interglacial period (Fig. 1). The loch site may then have been eroded when the sea retreated during the glaciation by the streams now entering its inner extremities, perhaps swollen by melt

water from the ice inland. In the immediate postglacial period the sea then rose again to cut another very distinct shoreline about 15 foot above the present one, and a series of shingle spits were progressively formed, extending south across the mouth of the resulting inlet, which was eventually closed by sand blowing up the coast from the south since the sea level returned to its present level about 5,000 yr ago. During the terminal stages of this process in the seventeenth century the harbour mouth at the south end became blocked. Rows of dunes extended north from Rattray Head, with a shallow inlet parallel to the coast. The lows of this inlet and the old channel now form dune slacks.

Limnology

The associated dunes are calciferous, and the loch is a moderately eutrophic water body with an alkalinity of 70 to 80 p.p.m. as CaCO₃ and a distinct saline influence now due to spray blown from the sea. As the loch is extremely shallow for its size the sandy bottom is brought into suspension during periods of high winds rendering the water turbid. Wave action has reduced much of the shoreline to wave-washed sand and gravel with only a limited amount of marginal vegetation, but in the more sheltered western arm there are some extensive beds of *Phragmites* with smaller patches elsewhere. Submerged vegetation, which forms one of the chief sources of food for wildfowl, is, however, plentiful, including *Chara aspera*, *Potamogeton filiformis* and *Littorella uniflora* in shallow water, with *Potamogeton pectinatus*, *P. perfoliatus* and *Cladophora* in deeper water. This plant assemblage is very characteristic of sandy eutrophic and particularly maritime lochs in Scotland.

The varied invertebrate fauna of the loch contains several species associated with very slightly brackish conditions, which occur rather infrequently in British lakes. The gastropod *Potamopyrgus jenkinsi* is extremely abundant and the amphipod *Gammarus duebeni* and the corixid *Sigara selecta* plentiful. The other invertebrates recorded are either species characteristic of a sandy substrate (for example *Caenis moesta*, *Arctocorisa germari* and *Sigara dorsalis*), wave-washed shores (for example *Tinodes waeneri* and *Oreodytes rivalis*) or weed beds in eutrophic conditions (for example *Cloeon simile*, Leptoceridae, *Sigara falleni* and *Callicorixa praeusta*). The zooplankton consists chiefly of the common association of *Daphnia hyalina*, *Cyclops strenuus* and *Diaptomus gracilis*. The abundance of invertebrates, particularly gastropods, forms a rich source of food for the trout, three-spined sticklebacks, flounders and eels, which are the only fish, and for some of the wildfowl.

Botany

The unusual geomorphological history of the area has resulted in the creation of a diversity of habitats of outstanding botanical interest. In addition to the loch and its

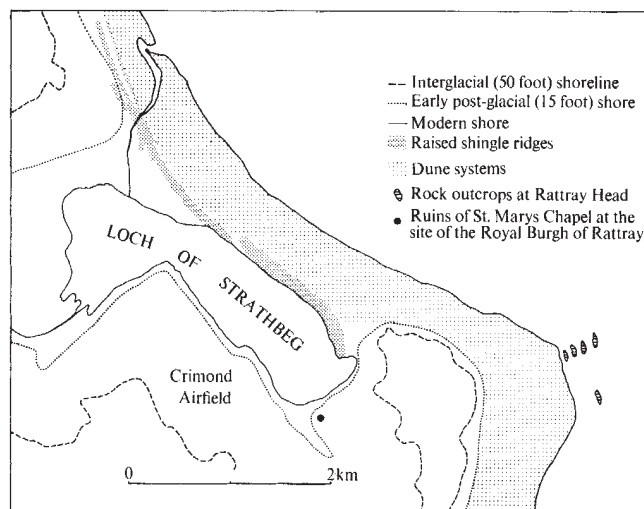


Fig. 1 The Loch of Strathbeg.

margins these comprise marshland, *Salix* scrub, salt marsh, dune slacks, and dunes built of sand containing a relatively high proportion of shell fragments. More than seventy species of vascular plants have been recorded within the boundary of the Site of Special Scientific Interest. These include one extremely localized species, *Ranunculus reptans*, which is confined to about five stations in the British Isles, several predominantly northern species, notably *Ligusticum scoticum*, here behaving rather atypically as a dune plant, and a few principally southern species of restricted distribution in north-east Scotland such as *Epilobium hirsutum*. This alone indicates that the locality is of special ecological significance.

In addition to the aquatic macrophytes already mentioned, attention may be drawn to the zonation of plant communities around the southern margin of the loch. Bordering on the dunes or fields is a zone dominated by *Phalaris arundinacea*, which includes such species as *Iris pseudacorus*, *Achillea ptarmica*, and *Myosotis scorpioides*. Nearer the loch, especially in the bays, this gives place to an *Eleocharis*

palustris zone, the upper part of which contains *Juncus articulatus*, *Leontodon autumnalis*, *Polygonum amphibium*, *Senecio aquaticus*, *Lotus uliginosus* and others. The lower part, around the edge of the water, is characterized by such species as *Carex nigra*, *Littorella uniflora*, *Myriophyllum alterniflorum*, *Equisetum fluviatile* and, where the cover is incomplete and most species are dwarf or decumbent, *Ranunculus reptans*. The latter is currently the subject of a taxonomic and autecological investigation by Mrs E. L. Birse.

To the north of the loch is an extensive, though narrow, area of marsh with a typical complement of species such as *Filipendula ulmaria*, *Hippuris vulgaris*, *Triglochin palustris* and *Carex rostrata*. Part of this has been colonized by *Salix cinerea* ssp. *atrocinerea* and *S. viminalis*, with *S. caprea*, *S. purpurea* and *Alnus glutinosa* in small numbers (J. W. Kinnaid, personal communication).

The forward margin of the dunes is steep, but the presence of *Agropyron junceiforme* suggests that in some cases sand accumulates at the foot of the ridge. All stages of dune development and surface fixation are in evidence, including formation of a species-rich *Festuca rubra* turf with *Linum catharticum*, *Anthyllis vulneraria*, *Erodium cicutarium*, *Gentianella campestris* and *G. amarella* among others. Dune slacks are present, although not extensively developed, and to the north there is a low valley containing a rather sandy type of salt marsh including *Festuca rubra*, *Atriplex glabriuscula*, *Glaux maritima*, *Honkenya peploides* and *Plantago coronopus*.

Ornithology

The Loch of Strathbeg has been known as a famous site for birds for at least 150 years. In the first full account of its ornithology, published in 1854, Thomas Edward⁵ lists most of the breeding species, and reports "that the common people who live thereabouts believe, and say it too, that all the birds of the world come there in the winter". In 1903 George Sim⁶ reported that it is "the finest resort of wild fowl on the east coast of Scotland". In 1953 the Misses Baxter and Rintoul⁷ recorded that it is "a wonderful sheet of water, large and shallow, holding a splendid variety of birds". Atkinson Willes describes why in his monograph⁸ "many

Table 1 Averages and Maxima Recorded during Monthly Wildfowl Counts at the Loch of Strathbeg, 1954-1972

Number of counts		August	September	October	November	December	January	February	March	April	Total
1954-1963: Period	A:	1	7	8	6	4	4	5	5	1	41
1964-1972: Period	B:	—	8	9	9	8	9	6	5	—	54
Species											Maximum
Whooper Swan	A:	0	8	179	275	26	48	38	19	0	630
<i>Cygnus cygnus</i>	B:	—	X	328	166	82	75	106	86	—	822
Mute Swan	A:	224	220	184	139	141	62	113	58	22	269
<i>Cygnus olor</i>	B:	—	250	217	204	208	140	67	52	—	354
Greylag Goose	A:	0	2	201	345	311	434	191	120	9	1,166
<i>Anser anser</i>	B:	—	1	1,856	1,644	708	1,107	1,008	733	—	4,500
Pink-footed Goose	A:	0	5	712	411	51	76	194	418	1,050	2,000
<i>Anser brachyrhynchus</i>	B:	—	1	2,254	1,548	317	1,111	610	1,483	—	4,600
Mallard	A:	90+	507	1,198	2,047	2,634	3,715	1,990	771	386	8,000
<i>Anas platyrhynchos</i>	B:	—	360	822	1,713	2,276	2,135	1,337	406	—	6,515
Wigeon	A:	250	188	778	696	800	826	508	203	200	2,500
<i>Anas penelope</i>	B:	—	208	666	389	557	568	1,262	255	—	2,150
Pochard	A:	0	41	229	399	424	73	100	56	0	800
<i>Aythya ferina</i>	B:	—	39	973	1,073	361	424	416	156	—	2,764
Tufted Duck	A:	139	105	127	110	125	45	80	65	40	366
<i>Aythya fuligula</i>	B:	—	123	392	441	194	244	88	54	—	1,060
Goldeneye	A:	0	X	37	46	19	27	59	51	31	120
<i>Bucephala clangula</i>	B:	—	8	46	93	103	75	86	59	—	518

X, Recorded, average less than one.

factors combine to make this one of the greatest centres for wildfowl in Britain: first, the loch lies within a half mile of the sea in the easternmost corner of Scotland, and is thus most attractive to incoming migrants; second, it is large enough, with an area of 550 acres, to provide a secure roost, yet shallow enough to afford intensive feeding grounds; and last, the natural remoteness of the place is enhanced by careful keeping". Bourne⁹ observed with radar that it was a focal point for bird movements in north-east Scotland, and he and Patterson¹⁰ have described how Common Gulls *Larus canus* among other species follow the coast to the vicinity of Rattray Head before setting out over the sea when migrating from Britain to Scandinavia in spring. Elkins and Williams¹¹ have also reported the results of a long series of observations of seabird movements past Rattray Head lighthouse.

The wildfowl have now been counted once a month in the winter for 19 yr by two observers, the late Miss E. Garden and then Mr John Edelsten. The average number and maximum recorded for species where the total count has exceeded 500 are compared in Table 1. These counts are influenced by several variables, including the difficulty of counting the birds on such a large sheet of water, the tendency of geese to go inland to feed and ducks to retire to the sea when disturbed, and the fact that most of the birds are passing migrants or hard-weather visitors which only stay for a short time, all of which tend to result in underestimates of the number of birds visiting the loch. It will be seen, however, that except in the case of two surface-feeding duck which are present in greatest numbers in hard weather, of which there has been little in recent years, all species have tended to increase markedly during the period of observation in spite of a certain amount of controlled shooting. In general it now seems safe to say that at least 10,000 birds are normally present together during at least some stage of autumn passage every year, and many times this total, possibly as many as 100,000, may pass through, so that the loch

provides a vital staging post for migrants in an area where in fact there are few.

The vicinity of the loch is also one of the most important areas for breeding birds in north-east Scotland, where southern species such as in the past the Yellow Wagtail *Motacilla flava flavissima* have their northernmost breeding stations and overlap with those of northern species such as the Dunlin *Calidris alpina*, and there are at least eight, possibly ten, species of breeding duck. Mr N. Picozzi reports that during the recent five-year British Trust for Ornithology "ornithological atlas" survey ninety-two species likely to be breeding were recorded in this 10 km square, of which seventy-nine were proved to breed, compared with a maximum of eighty and sixty-seven and an average of sixty-five and fifty for the other fourteen squares in the north-east quarter of Aberdeenshire.

We thank the Wildfowl Trust, British Trust for Ornithology and Royal Society for the Protection of Birds for various ornithological data, and various members of the North-eastern Environmental Liaison Group, but especially Professor Kenneth Walton of the Geography Department, Aberdeen University, and J. A. Forster for information and comments on a draft of this note.

- ¹ Anon, *Trans. Buchan Field Club 1887-1890*, 82 (no date).
- ² Cumine, J., *Trans. Buchan Field Club 1887-1890*, 114 (no date).
- ³ Milne, J., *Trans. Buchan Field Club 1898-1900*, 180 (no date).
- ⁴ Walton, K., *Scott. Geogr. Mag.*, 72, 85 (1956).
- ⁵ Edward, T., *Naturalist*, 1854, 239 (1854).
- ⁶ Sim, G., *The Vertebrate Fauna of "Dee"*, 36 (Aberdeen, 1903).
- ⁷ Baxter, E. V., and Rintoul, L. J., *The Birds of Scotland*, xvii (Edinburgh and London, 1953).
- ⁸ Atkinson Willes, G. L., *Wildfowl in Great Britain*, 212 (London, 1963).
- ⁹ Bourne, W. R. P., *Bird Migration in Scotland Studied by Radar*, manuscript deposited at the Edward Grey Institute, Oxford (1963).
- ¹⁰ Bourne, W. R. P., and Patterson, I. J., *Scott. Birds*, 2, 3 (1962).
- ¹¹ Elkins, N., and Williams, M., *Scott. Birds*, 7, 66 (1972).

Heavy Metals in British Waters

A. PRESTON

Ministry of Agriculture, Fisheries and Food, Fisheries Laboratory, Lowestoft

Neither the number of areas contaminated with heavy metals nor the extent of the contamination seem to have changed much between 1960 and 1970.

THE programme of research and development on heavy metals in the marine environment, conducted by the Ministry of Agriculture, Fisheries and Food, has two chief objectives: first, the establishment and continued monitoring of concentrations in fish and shellfish of commercial importance in Britain to provide a basis for estimates of human intake of heavy metals^{1,2} and, second, to provide data to assist an understanding of the behaviour of heavy metals when they are introduced into the marine environment—the factors determining their geographical distribution, or biological fate, including their effect, if any, on marine resources^{3,4} and the kinetics of their metabolism in representative marine

organisms⁵⁻⁷. The first objective concerns short-term requirements for assessment and control of the present situation, whereas the second should provide a sound basis for a long-term policy of control, including the proper utilization of the capacity of the sea to accept such materials.

The result of a pilot survey of selected heavy metal concentrations in British coastal waters conducted during 1969 and 1970 was published last year⁸. This survey, and others conducted in 1971, was based on the sampling of both seawater and selected biological indicator materials (Fig. 1). The results of seawater sampling (Table 1) revealed that certain areas have elevated concentrations of some metals by comparison with the general levels prevailing in other areas. More detailed sampling using seaweed as an indicator confirmed that among these (Table 2) were the eastern Irish Sea (high values for Cu, Mn, Ni, Pb and Zn), the Severn Estuary (high values for Cd, Cu, Ni, Zn), the north-east coast round Seaham Harbour (Cd, Pb and Zn), and the Clyde in the vicinity of Hunterston (Cu, Mn and Ni). In these areas association with drainage from large industrial conurbations might well be invoked as a principal source of

Table 1 Concentrations ($\mu\text{g l}^{-1}$) of Selected Heavy Metals in British Coastal Waters and the Adjacent North-east Atlantic (1970-71)

Element		British coastal waters					North-east Atlantic Area 4A
		Area 1	Area 2	Area 3	Area 4	Area 5*	
Zn	Geometric mean	2.0	3.0	4.2	2.0	3.3	3.0
	Range	1.2-3.8	0.8-9.0	2.3-7.5	1.3-3.4	0.8-8.8	1.4-7.0
	No. of observations	4	10	21	8	43	5
Fe	Geometric mean	0.3	0.09	0.18	—	0.06	—
	Range	0.06-1.3	0.03-0.6	0.06-1.9	—	0.06-1.5	—
	No. of observations	4	10	21	—	4	—
Mn	Geometric mean	0.32	0.53	1.95	0.18	1.5	0.06
	Range	0.10-0.49	0.15-2.6	0.22-14.6	0.02-0.49	0.24-15.9	0.03-0.09
	No. of observations	4	10	21	8	43	5
Cu	Geometric mean	0.46	0.59	0.66	0.34	0.82	0.26
	Range	0.23-1.29	0.18-3.75	0.28-0.98	0.19-0.62	0.24-1.9	0.05-0.80
	No. of observations	4	10	21	8	43	5
Ni	Geometric mean	0.38	0.38	0.71	0.53	0.84	0.43
	Range	0.22-0.95	0.22-0.55	0.32-22.9	0.36-0.79	0.16-5.4	0.29-0.66
	No. of observations	4	10	21	8	43	5
Pb	Geometric mean	0.17	0.19	0.11	<0.05	0.21	—
	Range	<0.05-1.1	<0.05-1.2	<0.05-1.0	—	<0.05-0.8	—
	No. of observations	4	10	21	8	4	—
Cd	Geometric mean	0.06	0.11	0.04	<0.01	0.20	0.04
	Range	<0.01-0.38	<0.01-0.52	<0.01-0.62	<0.01-0.18	0.01-1.4	<0.01-0.41
	No. of observations	4	10	21	8	43	5

* J. W. Dutton, A. R. Folkard, P. G. W. Jones, and D. F. Jefferies, to be published.

the contamination. Some of these areas have also subsequently been reported by other authors to be areas of high ambient metal concentration^{9,10}. Other relatively contaminated areas, not associated with such obvious pollution sources, were, however, found; there are high levels of Zn in the western Irish Sea, and of Cd, Pb and Zn off the coast of North Wales. Here the chief contribution may be attributed to drainage from mineralized catchment areas.

Several other interesting findings were also made. First, and perhaps most significant, an analysis of seaweed samples collected a decade earlier, at the same positions as those used for seaweed sampling in the current survey, showed

overall no significant differences in concentration with the exception of cadmium (Table 3). This suggests that the metal pollution situation as a whole has changed little, if at all, in location or magnitude during that period: indeed, the concentrations of cadmium may on average have fallen. Second, the current survey showed an attenuation of concentration offshore, the high concentrations being virtually a thin skin round the margins of the sea areas examined. The concentrations in open seawater, even of areas such as the Irish Sea and the North Sea, are not, except in isolated instances, significantly different from those in the open Atlantic Ocean adjacent to the British Isles (Table 1). Third, in the Irish Sea, where distributions with respect to depth were examined, no significant difference in metal concentration in filtered seawater samples was found between surface, midwater and bottom, indicating thorough mixing in this shallow sea area.

The later and more detailed examination of the situation in the North Sea carried out during 1971, including both

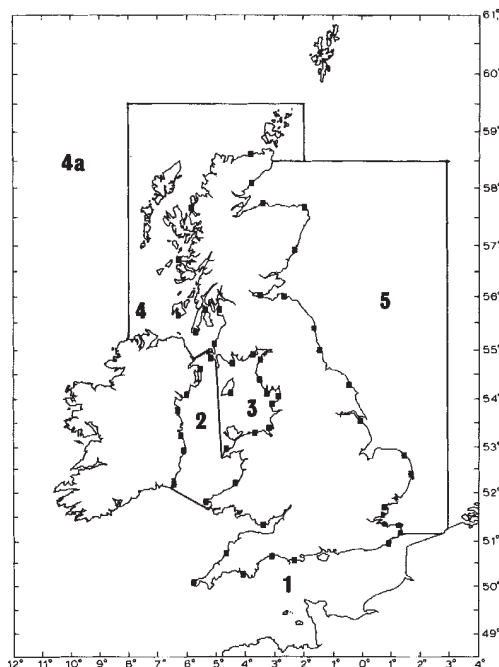


Fig. 1 Principal seawater sampling areas in British coastal waters. ■, Sampling points for *Fucus* spp.

Table 2 Derived Concentrations ($\mu\text{g l}^{-1}$) of Selected Metals in Shoreline Seawater of "Hot Spot" Areas, Based on Seaweed Sampling, Compared with North-east Atlantic Seawater (1970)

Location	Cd	Cu	Mn	Ni	Pb	Zn
Eastern Irish Sea	0.6	3.6	4.4	3.0	2.3	26.6
Severn Estuary	7.7	4.6	2.1	6.4	1.5	22.2
Tyne-Tees (Seaham Harbour)	0.8	3.3	2.5	2.7	2.2	11.7
Clyde (Hunterston)	0.6	2.8	8.2	3.0	1.8	6.2
Atlantic (cf. Table 1)	0.04	0.26	0.06	0.43	0.02*	3.0

* Open ocean concentration¹¹—value from IDDE Baseline Conference.

seawater and seaweed sampling, confirmed the interpretations of the previous survey, namely that in general elevated concentrations are restricted to the margins and to confined localities largely associated with industrial development. It also showed that the North Sea coastal water was on average about as contaminated as the east Irish Sea, the area previously revealed as having the highest average concentrations of most of the metals examined. Elements of this

North Sea programme are being conducted as part of an international study of pollution in the North Sea under the auspices of the International Council for the Exploration of the Sea. The study, which is being jointly carried out by

Table 3 Comparison of Heavy Metal Concentrations in *Fucus* Seaweed from Britain (1961 and 1970)

Element	Geometric mean quotient	<i>t</i>	<i>P</i>
Cd	0.68	3.32	>0.01
Cu	0.90	1.34	<0.2
Fe	1.03	0.27	<0.7
Mn	0.86	1.68	<0.1
Ni	0.94	0.70	<0.4
Pb	0.82	1.28	<0.2
Zn	0.90	0.94	<0.3

Significance testing of the logarithm of the quotient metal concentration (1970)/metal concentration (1961).

member states whose coastlines abut the North Sea, was undertaken at the initiative of the Ministry of Agriculture, Fisheries and Food following a request from the United Nations Food and Agriculture Organization Technical Conference on Marine Pollution. Apart from the work already described, this study also includes an examination of heavy metal concentrations in representative commercial fish species and a compilation of sources and magnitudes of inputs. Other aspects are concerned with pesticides and halogenated hydrocarbons.

The data from British coastal waters and the results of the monitoring of commercial fish species have also been contributed to the Environmental Quality Programme of the United States International Decade of Ocean Exploration (IDOE). This programme, which is supported by the National Science Foundation, is part of the overall United

data¹¹ are very similar, namely, elevated concentrations are largely restricted to estuaries and the narrow coastal margin, and are associated with drainage from industrial areas or the dumping of sewage sludge and industrial wastes. It has been recommended that the United States programme should place continuing emphasis on research in coastal waters and estuaries by virtue of their elevated metal concentrations and the importance of the fishery resources involved.

In summary, the results of seawater sampling on a pilot scale around the British Isles show that contamination is restricted to a few areas, chiefly, but not exclusively, linked to industrial development, and that even in these areas contamination does not spread very far offshore. Neither the number of contaminated areas nor the magnitude of the contamination seems to have changed much between 1960 and 1970. Furthermore, the monitoring of commercial fish species¹² shows that, except in the case of sedentary shellfish, variations in inshore seawater metal concentrations are not reflected to any great extent in the edible portions of these species (Table 4). Fish are mobile, and the timescale of their metal turnover smooths out temporal and spatial variations in seawater concentrations. Of course concentrations in inshore fish are sometimes higher than those in offshore fish, especially in the case of Cd, Hg and Pb. This strongly indicates that metal pollution problems are essentially national or regional problems, with the possible exception of the lead problem as atmospheric transportation is an important contributory factor. Their control and monitoring can thus best be met by national or perhaps regional action. The deep-sea dumping of waste is a minor contribution to metal pollution and, moreover, is likely to be brought under international control very soon¹³⁻¹⁵.

In the future, attention will continue to be directed towards the monitoring of pollutant concentrations and inputs, in order to provide a basis for regulating discharges to a level commensurate with acceptable concentrations in those biological materials judged to be critical in the context of public health or resource damage. There is also an outstanding need for better data about acceptable standards for human and resource exposure, and in particular better methods for assessing the significance of metal concentrations for marine organisms at concentrations appropriate to the environmental situation, that is long-term exposure to low concentrations, need to be developed.

Table 4 Mean Concentrations ($\mu\text{g g}^{-1}$ Wet Weight) of Metals in the Muscle of Fish Landed in England and Wales from Selected Sea Areas

Origin	Cd	Cu	Hg	Pb	Zn
English and Welsh coasts (less than 25 miles from the coast)	0.11	0.95	0.29	0.66	5.52
Irish Sea (more than 25 miles from the coast)	0.07	0.80	0.21	0.50	5.70
North Sea (more than 25 miles from the coast)	0.12	<0.50	0.10	<0.50	4.83
Distant water (Iceland, Barents Sea, Norway Coast)	<0.05	1.80	0.06	<0.50	6.10

States contribution to the Intergovernmental Oceanographic Commission's long-term and expanded programme of oceanic exploration and research (LEPOR), aspects of which are concerned primarily with marine pollution, which was recognized, when formulating the programme, as an important area requiring further research.

The IDOE environmental quality programme has so far been largely concerned with establishing pollutant baselines for the Pacific, Gulf and Atlantic coasts of the United States. The British data on metals were contributed to provide a complementary picture for part of the eastern seaboard of the Atlantic. The general conclusions from both sets of

¹ *Survey of Mercury in Food* (HMSO, London, 1971).

² *Survey of Lead in Food* (HMSO, London, 1972).

³ Portmann, J. E., *MAFF Shellfish Information Leaflet No. 19* (Ministry of Agriculture, Fisheries and Food, 1970).

⁴ Portman, J. E., and Wilson, K. W., *MAFF Shellfish Information Leaflet No. 22* (Ministry of Agriculture, Fisheries and Food, 1971).

⁵ Preston, A., Jefferies, D. F., and Pentreath, R. J., *Conservation and Productivity of Natural Waters*, Symp. Zool. Soc. Lond., **29**, 271 (1972).

⁶ Pentreath, R. J., *Proc. IAEA Symp. on Interaction of Radioactive Contaminants with Constituents of the Marine Environment*, Seattle (in the press).

⁷ Pentreath, R. J., *J. Mar. Biol. Ass. U.K.* (in the press).

⁸ Preston, A., Jefferies, D. F., Dutton, J. W. R., Harvey, B. R., and Steel, A. K. (in the press).

⁹ Abdullah, M. I., Royle, L. G., and Morris, A. W., *Nature*, **235**, 158 (1972).

¹⁰ Bellamy, D., *New Scientist*, **54**, 76 (1972).

¹¹ *Baseline Studies of Pollutants in the Marine Environment and Research Recommendations: The IDOE Baseline Conference* (New York, May 1972).

¹² Portman, J. E., *Aquaculture*, **1**, 91 (1972).

¹³ *Convention for the Prevention of Marine Pollution by Dumping from Ships and Aircraft*, Oslo, February 15, 1972, Cmnd 4984 (HMSO, London, 1972).

¹⁴ *Identification and Control of Pollutants of Broad International Significance; United Nations Conference on the Human Environment*, Stockholm, June 1972, 125 (A/CONF. 48/8).

¹⁵ *Final Act of the Intergovernmental Conference on the Convention on the Dumping of Wastes at Sea*, London, November 13, 1972; Cmnd 5169 (HMSO, London, 1972).

Descent of Lithosphere beneath New Hebrides, Tonga-Fiji and New Zealand: Evidence for Detached Slabs

MUAWIA BARAZANGI, BRYAN L. ISACKS & JACK OLIVER

Department of Geological Sciences, Cornell University, Ithaca, New York 14850

JACQUES DUBOIS & GEORGES PASCAL

Office de la Recherche Scientifique et Technique, Outre-Mer, Noumea, New Caledonia, and Institut de Physique du Globe, Université de Paris, 6 Paris

Study of seismic wave propagation in the mantle beneath the New Hebrides island arc shows that the remarkable gap in seismic activity between deep and intermediate depth earthquakes at the northern part of the arc corresponds to a gap in the lithospheric slab descending beneath the arc: the deep earthquakes mark a detached piece of lithosphere. Although observations for New Zealand deep earthquakes are ambiguous, other evidence suggests the detachment of lithosphere beneath New Zealand.

ONE of the outstanding features of the distribution of earthquakes in the upper mantle is the existence of gaps in seismic activity between depths of about 300 and 550 km. These gaps are prominent beneath South America, New Zealand, and the New Hebrides island arc. They are of great interest because of the implication that portions of lithosphere can break off from the descending plate and exist as isolated slabs within the mantle.

Here we present evidence from study of seismic wave attenuation for the detachment of lithospheric slabs in the upper mantle beneath the New Hebrides arc. Detachment is also indicated by travel time data and reconstructions of the past movements of plates in the region. We concentrate on the study of seismic attenuation because the effects are large and easy to observe and interpret¹⁻³.

Isacks and Molnar⁴ suggest that the gaps in seismic activity as a function of depth can be explained by two models. In the first the stress inside a continuous slab varies from down-dip extension at intermediate depths to down-dip compression at greater depths, and thus is near zero between these depths. In the second a piece of descending lithosphere breaks off, sinks into the upper mantle and leaves a gap between the piece and the plate to which it was attached. The character of seismic waves, especially shear waves, produced by deep earthquakes and recorded at stations along island arcs where large gaps in seismic activity exist provides evidence for determining which of these models is correct. Observation of attenuated, low frequency waves for the appropriate paths indicates a gap

in the descending lithosphere. But if high frequency shear waves are observed, then the interpretation is ambiguous. The observations can be explained either by propagation along a continuous high Q lithospheric slab or by propagation through the upper portion of a discontinuous slab that provides a low attenuation path through the asthenosphere. The second explanation implies that for 1-3 Hz shear waves attenuation is small below depths of about 300 km.

The New Hebrides island arc provides a unique opportunity to study the nature of the gap in seismic activity in the upper mantle. Seismograph stations are well distributed along the arc. The gap is well established between depths of about 300 and 600 km. The structure of the intermediate and deep seismic zones with the very steeply dipping intermediate zone and the approximately horizontal deep zone certainly suggests that the two zones may not be connected. Enough deep earthquakes have occurred since the stations were installed to provide a large sample of paths. Forty deep earthquakes in the 10 year period 1961-1970 were recorded at Noumea (NOU) and Port Vila (PVC) stations (Fig. 1). Lonerore (LNR) was established in 1968. NOU, PVC and LNR employ 1 Hz underdamped seismometers and 2 Hz overdamped galvanometers. The instruments have a relatively flat response for seismic wave frequencies between 0.5 Hz and 10 Hz and thus record very clearly the large variations of shear wave fre-

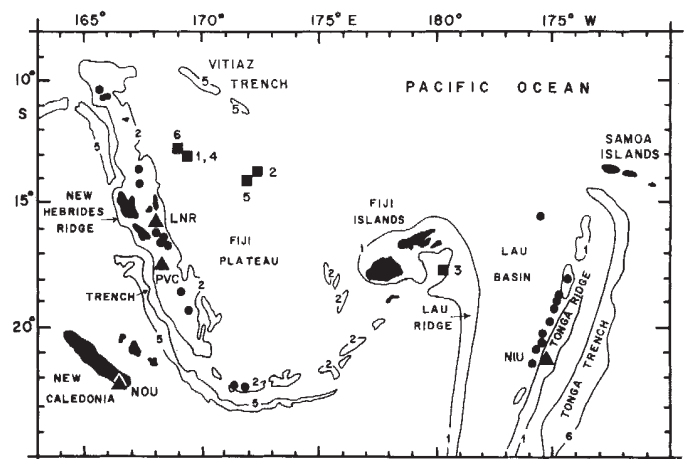


Fig. 1 Map showing the Tonga-Fiji-New Hebrides region of the southwest Pacific. ●, Historically active volcanoes; ▲, seismic stations; ■, locations of deep earthquakes used in Figs. 2 and 3. Water depths are in km.

quencies. Detailed description of the New Hebrides–New Caledonia seismic network is given by Dubois⁵.

The inclined seismic zones of South America also have remarkable gaps in seismic activity between about 350 and 550 km. A detailed study of the records produced by almost all the South American deep earthquakes that occurred during the past 10 yr at stations along the western coast of South America is currently under way, and will be reported in a separate study.

Lithospheric Gap in New Hebrides Arc

Fig. 1 shows the location of the stations in New Caledonia and New Hebrides used in this study. We have examined all the records produced at these stations by the New Hebrides deep earthquakes. The most striking observation is that predominantly low frequency (about 0.5 Hz) S waves are recorded at PVC and LNR from the deep earthquakes north of 15° S.

Fig. 2 shows a cross-section through the New Hebrides arc that intersects NOU, LNR and passes close to PVC. New Hebrides deep earthquakes located at the western part of the deep zone (very close to the downward projection of the intermediate depth zone) produce attenuated, low frequency S waves at PVC. The ray paths pass just beneath the dipping seismic zone. Frequencies greater than 1 Hz are absent and the amplitude of the S phase is generally less than that of P phase. As Oliver and Isacks¹ and Barazangi and Isacks³ show, this can be explained by a transmission through an attenuating low *Q* zone. In contrast, S waves recorded at NIU station on the Tonga island arc from Tongan deep earthquakes have predominant frequencies of 3–4 Hz and the amplitudes of S are generally larger than those of P.

Deep earthquakes also produce low frequency shear waves at LNR, a station close to the active volcanic line of the New Hebrides arc. This is in marked contrast to the observation of high frequency S waves from Tonga deep earthquakes at stations along the active volcanic line of the Tonga arc⁶.

We interpret the observation of low frequency S waves at PVC and LNR to be mainly the result of attenuation along the path. The effect of the source can be excluded since New Hebrides deep earthquakes produce seismograms at Fiji stations that are similar to those produced by the Tonga deep earthquakes (the New Hebrides and Tonga deep earthquakes are approximately equidistant from the Fiji stations). The effect of the station can be excluded since intermediate earthquakes located along the New Hebrides arc produce, without exception, high frequency (about 3–4 Hz) S waves at PVC and LNR. This is so even where the path lengths are comparable or greater than those from the deep earthquakes. Further, the Tonga deep earthquakes produce seismograms at PVC and LNR that are strikingly similar to those produced at the Fiji stations. Thus, attenuation along the path is the main cause for the observed low frequency S waves at PVC and LNR.

The attenuation is chiefly below 300 km, judging by the abundant observations of high frequency shear waves from intermediate depth earthquakes to PVC and other New Hebrides stations. All observations taken together are best explained by the absence of lithospheric slab material between depths of about 300 and 600 km; the deep earthquakes of New Hebrides therefore represent a detached slab in the upper mantle. To our knowledge, this is the first direct evidence that the attenuating, asthenospheric layer is deeper than about 300 km in the upper mantle.

New Hebrides deep earthquakes always produce low frequency S waves at NOU (Fig. 2). This is most probably due to attenuation in the upper mantle (the asthenospheric layer), because the ray paths to NOU completely miss the dipping New Hebrides seismic zone.

New Hebrides deep earthquakes located at the eastern part of the deep zone produce attenuated, low frequency S waves at PVC and LNR. The ray paths, calculated for a laterally

homogeneous mantle with a Jeffreys–Bullen (J–B) velocity structure, pass just above the inclined seismic zone (Fig. 3). S phases which similarly appear to pass above the Tonga inclined zone, however, have large amplitudes and high frequencies (we note that the time scale of NIU record is about twice that of PVC, as shown in Fig. 3). Even though the J–B ray path seems to miss the Tonga seismic zone, the high frequency S waves probably travel through the descending slab. Barazangi, Isacks and Oliver⁷ describe other evidence that the slab descending beneath Tonga acts as a wave guide for high frequency shear waves and is therefore continuous. Thus by comparison the absence of these high frequency shear waves for the easternmost New Hebrides deep earthquakes can be taken as evidence for the detachment of lithosphere beneath the New Hebrides.

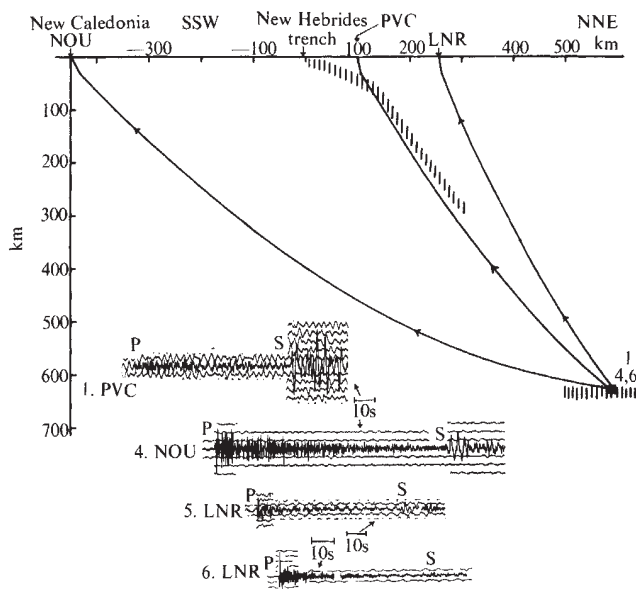


Fig. 2 Cross-section of the New Hebrides arc showing J–B ray paths to NOU, PVC and LNR stations and the corresponding records [E–W component at PVC, N–S component at NOU, and Z component at LNR] and the locations of seismic activity (vertical lines). Only low frequency S waves are recorded at the stations.

We will present a detailed study of the travel times of P waves of the New Hebrides deep earthquakes later. The travel time residuals of P waves of the New Hebrides deep earthquakes at PVC and LNR stations along the arc are close to normal (about 1 to 2 s earlier). This is in contrast to P residuals of about 4 to 5 s earlier from the Tongan deep earthquakes recorded at stations along the Tonga arc⁸. Thus the travel time data support those obtained from seismic wave attenuation and indicate that the deep earthquakes at the northeast of the arc represent a detached lithospheric slab.

During the past seven years three deep earthquakes occurred south of 15° S, south of the horizontal oblong-shaped zone, and are located along a line parallel to the southern part of the New Hebrides arc (Fig. 5). Records produced by these earthquakes at NOU in New Caledonia and at stations located in the northern part of the New Hebrides arc (to the north of about 16° S latitude, LNR and LUG) show attenuated, low frequency S waves. But records produced at PVC from the most recent event located at about 18° S and 173° E show large amplitude, high frequency S waves. This is the only one of the three shocks recorded at PVC with good quality records. This observation is quite clear, however, and may imply the continuity of the descending slab in the southern part of the New Hebrides arc. More data are required before a meaningful interpretation can be made for the southern deep earthquakes zone.

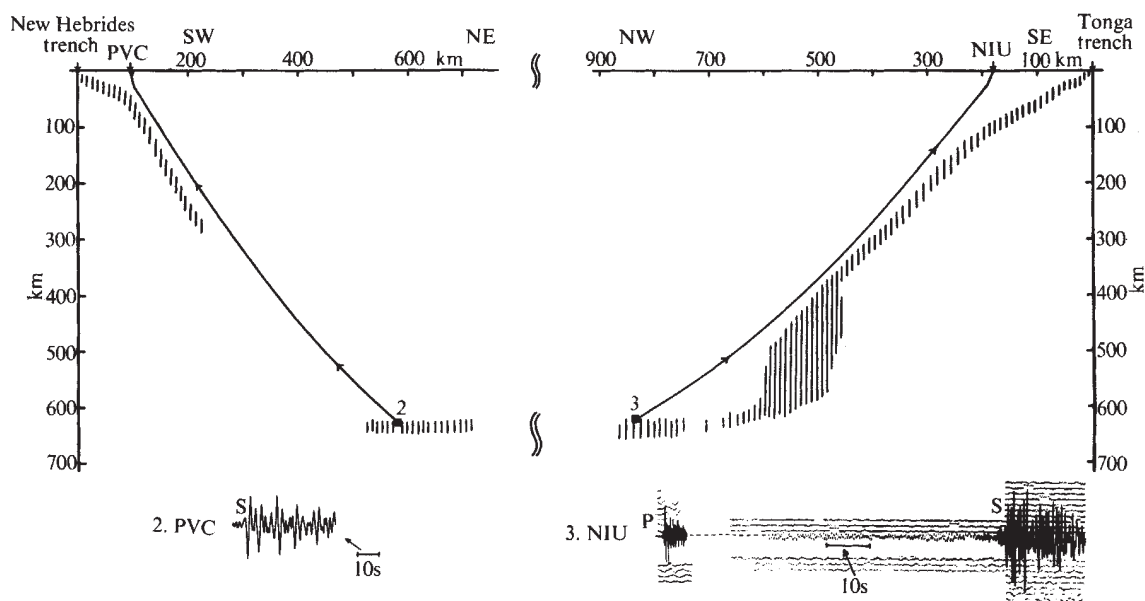


Fig. 3 Two cross-sections of Tonga and New Hebrides arcs showing ray paths to NIU and PVC stations and the corresponding records [E-W component at PVC, and Z component of P, and N-S component of S at NIU] and the locations of seismic activity in the upper mantle (vertical lines). Note the great difference in the signature of S waves at NIU and PVC in spite of the similarity in ray paths (time scale of NIU record is about twice that of PVC).

Detached Piece of Lithosphere beneath New Zealand?

In New Zealand earthquakes reach a depth of about 300 km in the North Island and about 200 km in the northernmost part of the South Island. In addition three earthquakes occurred at depths of about 600 km in 1953 and 1960 beneath the North Island⁹. Fig. 4 shows a cross-section of the New Zealand arc and examples of seismograms from the local New Zealand network. Deep earthquakes produce high frequency S waves at Wellington (WEL). Two quite different explanations can be made for this. One is that the slab beneath New Zealand is continuous and reaches depths of at least 600 km, and thereby provides a path for high frequency S waves. The second is that although the slab may have a gap beneath about 300 km, the portion above 300 km is sufficient to provide a "window" through the zone of high attenuation. This second explanation implies that the principal zone of attenuation is located above 300 km, and thus implies a significant difference between the New Zealand and New Hebrides regions with respect to attenuation below 300 km.

The second alternative, that a detached lithosphere is present beneath the North Island of New Zealand, is supported by the variation of down-dip length of the inclined seismic zone as a function of latitude along the New Zealand-Kermadec-Tonga plate boundary. If New Zealand deep earthquakes are excluded, the down-dip length increases regularly northward as predicted by locations of the pole of relative motion between the Australian and Pacific plates^{10,11}. The New Zealand deep shocks are thus distinctly anomalous in this respect and seem to mark a detached piece of plate.

New Zealand deep earthquakes produce low frequency, attenuated S waves at Tarata (TNZ) and Onerahi (ONE) which are located to the west of the line of active volcanoes in the North Island. Mooney¹² mapped a zone of anomalously high attenuation in the uppermost mantle also located west of the active volcanoes. Thus, the low frequency S waves at TNZ and ONE are probably the result of attenuation in the uppermost mantle to the west of the dipping seismic zone.

An interesting observation is that New Zealand deep shocks produce high frequency S waves at stations in the South Island. Fig. 4 shows an example recorded at Gebbies Pass (GPZ), a station located along the aseismic¹³ east coast of

the South Island. This observation suggests that beneath at least the northern part of the South Island no major zone of attenuation is present. The lithosphere marked by intermediate depth earthquakes beneath the North Island may thus extend beneath part of the South Island.

Deep Earthquakes beneath Fiji

An unusual feature of the earthquake distribution in the Tonga-Fiji region is the occurrence of deep earthquakes west

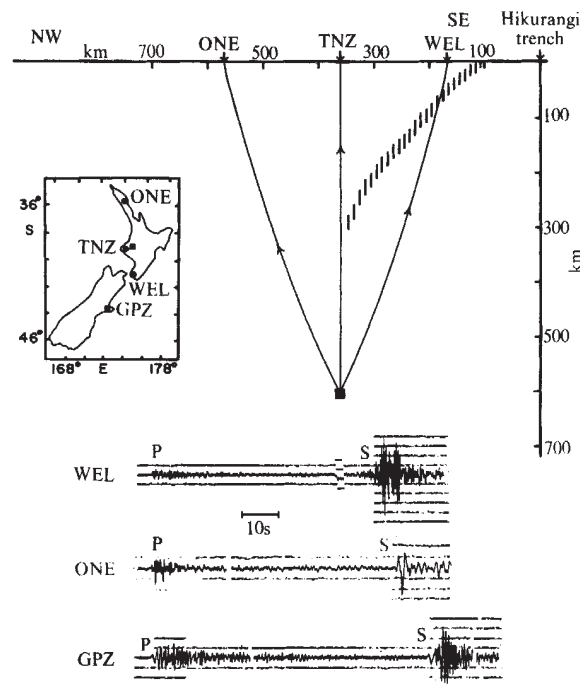


Fig. 4 Cross-section of the New Zealand arc showing ray paths to WEL, TNZ and ONE stations, horizontal records to WEL, ONE and GPZ stations, and the dipping seismic zone beneath the North Island (vertical lines). Insert map shows the locations of stations (●) and the location of the deep earthquakes of New Zealand (■).

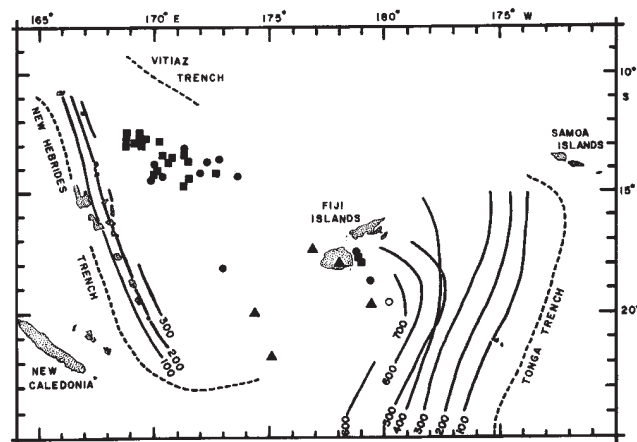


Fig. 5 Map showing contours of earthquake depths for Tonga and New Hebrides arcs, the deep seismic zone of New Hebrides, and well located deep earthquakes beneath the Fiji islands and the Fiji plateau. Δ , Events with depth range between 525 and 575 km; $\bullet$, events with 576 to 625 km depth; $\blacksquare$, events with 626 to 675 km depth. The open circle represents an event with a depth of 470 km.

of the inclined seismic zone of the Tonga arc (Fig. 5). In the past 10 yr about 8 well located deep earthquakes occurred beneath the Fiji Islands. It is not clear whether these earthquakes represent a continuation of the descending Tonga slab, a slab(s) detached from the present descending slab, or a slab detached during an earlier episode of underthrusting. Evidence obtained from focal mechanisms by Isacks *et al.*¹⁴ suggests that these earthquakes represent a contorted continuation of the northern edge of the descending Tonga slab. In any case these earthquakes and the New Hebrides deep earthquakes show a considerable horizontal extent away from the corresponding descending slabs. This suggests that the earthquakes mark lithospheric slabs that are unable to penetrate the 600–700 km discontinuity of the upper mantle, and hence the

discontinuity may represent the lower limit of the asthenosphere. These slabs may pile up above the discontinuity until their assimilation in the mesospheric lower mantle.

Overall Model

The New Hebrides and New Zealand deep earthquakes mark detached pieces of lithosphere in the upper mantle; considerable seismic wave attenuation probably exists below about 300 km of depth in the upper mantle beneath the northern part of the New Hebrides arc, which implies that the asthenosphere may extend deeper than 300 km in the mantle in this region; the spatial distribution of deep earthquakes between the New Hebrides and Tonga arcs suggests that slabs of lithosphere are unable to descend beneath about 700 km in the mantle.

We thank R. D. Adams for original seismograms, Walter Mitronovas for discussions, and Jim Gill and Dan Karig for preprints of their research. M. B. thanks Maurice Ewing for a research grant.

This work was supported by National Science Foundation grants.

Received October 24, 1972.

- ¹ Oliver, J., and Isacks, B., *J. Geophys. Res.*, **72**, 4259 (1967).
- ² Molnar, P., and Oliver, J., *J. Geophys. Res.*, **74**, 2648 (1969).
- ³ Barazangi, M., and Isacks, B., *J. Geophys. Res.*, **76**, 8493 (1971).
- ⁴ Isacks, B., and Molnar, P., *Rev. Geophys. and Space Phys.*, **9**, 103 (1971).
- ⁵ Dubois, J., *J. Geophys. Res.*, **76**, 7217 (1971).
- ⁶ Mitronovas, W., Isacks, B., and Seeber, L., *Bull. Seismol. Soc. Amer.*, **59**, 1115 (1969).
- ⁷ Barazangi, M., Isacks, B., and Oliver, J., *J. Geophys. Res.*, **77**, 952 (1972).
- ⁸ Mitronovas, W., and Isacks, B., *J. Geophys. Res.*, **76**, 7154 (1971).
- ⁹ Adams, R., *N.Z. J. Geol. Geophys.*, **6**, 209 (1963).
- ¹⁰ LePichon, X., *J. Geophys. Res.*, **73**, 3661 (1968).
- ¹¹ Chase, C., *Bull. Geol. Soc. Amer.*, **82**, 3087 (1971).
- ¹² Mooney, H., *J. Geophys. Res.*, **75**, 285 (1970).
- ¹³ Hamilton, R., and Gale, A., *J. Geophys. Res.*, **73**, 3859 (1968).
- ¹⁴ Isacks, B., Sykes, L., and Oliver, J., *Bull. Geol. Soc. Amer.*, **80**, 1443 (1969).

Effect of Magnesium on Horizontal Cell Activity in the Skate Retina

JOHN E. DOWLING & HARRIS RIPPS

The Marine Biological Laboratories, Woods Hole, Massachusetts 02543; the Biological Laboratories, Harvard University, Cambridge, Massachusetts 02138; and the Departments of Ophthalmology and Physiology, New York University School of Medicine, New York 10016

Magnesium, like light, hyperpolarizes retinal horizontal cells. In the dark, horizontal cells seem to be depolarized by neurotransmitter released continuously from photoreceptors.

THERE is both anatomical and physiological evidence to suggest that synaptic transmission between vertebrate photoreceptors

and second-order retinal neurones (horizontal and bipolar cells) is chemically mediated. The aggregation of vesicles in the receptor terminals¹, the separation between junctional membranes^{2,3}, and the latency differences between pre- and postsynaptic responses⁴ are consistent with this view. Although identification of the neurotransmitter has yet to be made, studies on the properties of horizontal cells have led to the interesting suggestion that the photoreceptor releases a depolarizing transmitter in the dark and that light decreases the flow of this substance^{5–8}. For example, horizontal cells exhibit low resting potentials (25–40 mV)^{9,10}, and most of

these cells only hyperpolarize in response to light⁹. Furthermore, an increase of input resistance of the horizontal cell often accompanies light stimulation¹¹, and transretinal currents that depolarize the receptor terminals induce a depolarizing response in the horizontal cells⁵⁻⁸. Recent studies on vertebrate photoreceptors have also provided results compatible with this notion. In darkness there is a steady inward sodium flux across the plasma membrane of the rod outer segment; light decreases the sodium conductance of the outer segment, thereby hyperpolarizing the receptor^{8,11,12}. Thus, the receptors are partially depolarized in the dark, a finding consistent with the suggestion of neurotransmitter release in darkness.

We have tested this hypothesis by determining the influence of magnesium on the electrical properties of skate horizontal cells (either *Raja erinacea* or *R. ocellata*). We assumed that, as at other chemically-mediated synapses, high levels of extracellular magnesium block neurotransmitter release from the presynaptic (that is receptor) terminal¹³⁻¹⁵. Intracellular recordings from horizontal cells were obtained from the eyecup preparation with glass micropipettes filled with 2 M KCl and having resistances of 35-55 Mohm when measured extracellularly in the tissue¹⁰. Signals were led from the micropipettes to a Bak ELSA-4 negative-capacitance amplifier via Ag-AgCl electrodes, observed on an oscilloscope, and recorded on a Brush (model 280) penwriter. In addition, recordings of the electroretinogram (ERG) were obtained with a partially insulated Ag-AgCl wire placed in the vitreous humour. ERG responses were amplified, displayed and recorded conventionally^{16,17}. After impaling a horizontal cell, a drop of elasmobranch Ringer¹⁸, in which 100 mM MgCl was substituted for 100 mM of NaCl, was gently pressure-ejected onto the surface of the retina. Recordings were obtained before, during, and after exposure to the high magnesium Ringer.

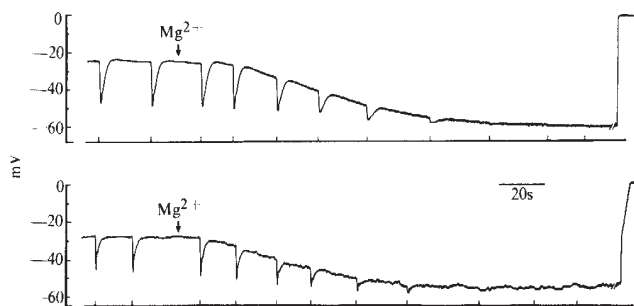


Fig. 1 Two experiments showing the effects of magnesium on skate horizontal cells. A drop of Mg-Ringer was applied to the eyecup (arrows), and within 15-25 s the cells began to hyperpolarize. Over the next 3 min, the cells hyperpolarized to a level of about -60 mV and light evoked activity was lost. A break in the trace indicates the time at which the pipettes were withdrawn from the cells. The rapid, positive shifts of potential of 55 to 60 mV confirmed the increase in membrane potential of the horizontal cells in the presence of high levels of magnesium. Flash intensity (filter density of 5.5) and duration (0.2 s) were kept constant throughout both experiments. The markers along the lower trace of each record indicate flash preparations.

Fig. 1 shows the effects of the test solution (Mg-Ringer) on two horizontal cells. Resting potentials of 25 and 30 mV were recorded initially in the two cells and light flashes of constant duration and intensity gave hyperpolarizing responses of 15-20 mV. The test stimuli were about 1.5 log units above threshold; flashes of saturating intensity evoked responses of 25-30 mV in these cells.

About 15-25 s after a drop of Mg-Ringer was applied to the eyecup (arrows, Fig. 1), the membrane potentials of the horizontal cells began to hyperpolarize and there was a corresponding decrease in the amplitudes of the light-evoked responses. Within 3 min, the membrane potentials had fallen to about -60 mV and the response to light was abolished. In these two experiments, the micropipettes were withdrawn from the cells after the resting potentials had stabilized. Positive shifts of potential of 55-60 mV signalled the exit of the pipettes from the cells and confirmed the intracellular measurements of large membrane potentials in the presence of high magnesium.

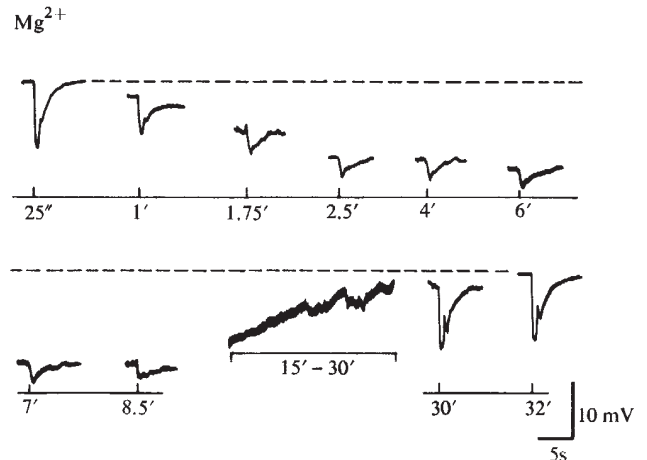


Fig. 2 Long-term recordings illustrating the transient nature of the effects of magnesium on skate horizontal cells. The times after the application of the test solution at which light stimuli were presented are indicated along the lower trace. The level of each trace relative to the dashed line shows the extent to which the cell was hyperpolarized. The cell was not completely hyperpolarized by the drop of test solution, so a small response could be elicited even after 8.5 min. Between 15 and 30 min (shown on the contracted time base) the membrane potential of the cell depolarized, and the responses at the end of this time had returned to normal amplitudes.

These results strongly support the notion that skate horizontal cells are maintained in a partially depolarized state in the dark, because of continual release of a depolarizing neurotransmitter from the photoreceptors. On this view, high magnesium, like light, decreases the release of neurotransmitter and the horizontal cell hyperpolarizes. As the horizontal cells hyperpolarize in response to a high magnesium environment, there is a distinct increase in baseline fluctuations. This is particularly evident in the lower record of Fig. 1. It is possible that these fluctuations indicate the presence of spontaneous miniature postsynaptic potentials in the horizontal cells, which are uncovered in conditions of high extracellular magnesium. In muscle it has been shown that miniature endplate potentials persist in the presence of high magnesium^{19,20}.

The effects of a drop of Mg-Ringer on the eyecup preparation are transient. Fig. 2 illustrates an experiment in which a horizontal cell was monitored for more than 30 min after the test solution was added to the eyecup. In this experiment the horizontal cell did not fully hyperpolarize and light-evoked responses were not entirely abolished. After application of the Mg-Ringer for 30 s, the membrane potential of the cell became more negative and responses to light decreased in amplitude.

After 3–5 min the resting potential stabilized, and only small light-evoked potentials could be recorded. For the next 5–10 min, no significant changes were noted. Between 15 and 30 min, however, the horizontal cell membrane depolarized to its former level (~ -30 mV) and light-evoked activity regained its original amplitude.

Earlier experiments by Svaetichin and his colleagues^{21,22} showed that anoxia and a variety of metabolic poisons produce effects on horizontal cells similar to those reported here. Some of these agents may exert their effects by disrupting synaptic transmission, but others may inactivate the receptors as well. The ERG was not monitored in those experiments; no information on receptor activity in the presence of the agents was provided.

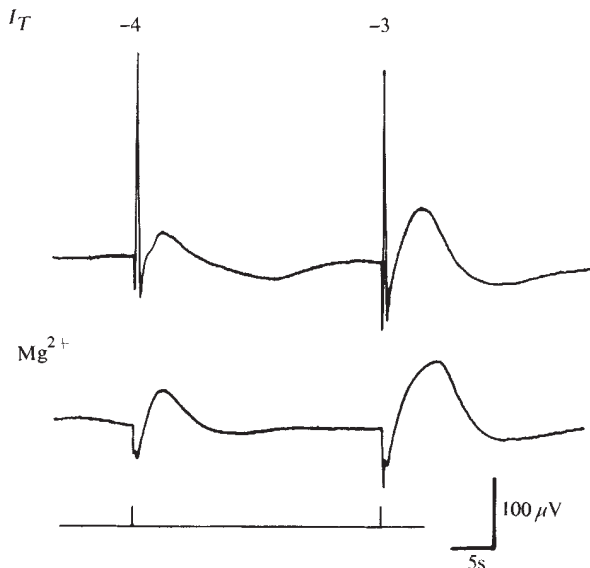


Fig. 3 The effects of Mg-Ringer on the skate electroretinogram (ERG). The upper traces show the principal components of the response at two flash intensities (filter densities of 3 and 4) in the normal ERG. The initial negative deflexion (a-wave) is followed by the large positive b-wave and a slow c-wave of lower amplitude. Magnesium almost entirely abolished the b-wave, but a- and c-waves are virtually unaltered.

Fig. 3 shows ERG recordings at two intensities ($I_T = -3$ and -4) before and 15–20 min after the Mg-Ringer was applied to the eyecup. The a- and c-waves show little alteration in the presence of high magnesium, but the b-wave is almost entirely abolished²³. The leading edge of the a-wave of the ERG is derived from photoreceptors²⁴, whereas the c-wave originates in the pigment epithelium²⁵ but depends on receptor activity for its generation²⁶. The b-wave, on the other hand, arises proximally to the receptors^{24,27}; its selective loss in the presence of high magnesium is to be expected if transmission between photoreceptors and second-order neurones is blocked. In some experiments, a small loss in amplitude of a- and c-waves was noted; this was probably due to the one-third reduction in Na^+ concentration in the Mg-Ringer²⁸. As we have said, the effects of a drop of Mg-Ringer on the eyecup preparation are transient; the tiny b-waves seen in the responses obtained 15–20 min after the test Ringer was applied to the preparation represent the initial stages of b-wave recovery.

The likelihood that vertebrate photoreceptors secrete a depolarizing transmitter in darkness raises two questions. The

first concerns the mechanisms by which bipolar cell responses of opposite polarity are produced by the action of a depolarizing transmitter. The hyperpolarizing bipolar cells exhibit an increase in membrane resistance during light stimulation^{8,29}, and thus generation of these responses may be similar to that of horizontal cell potentials. The depolarizing bipolar cells pose a more difficult problem. Nelson has recently found that the photic responses of these cells in the mudpuppy (*Necturus*) are accompanied by a decrease in membrane resistance²⁹. If these bipolar cells also receive their input directly from receptors, this finding implies that the effect of the neurotransmitter is to decrease conductance of the cell. Although unconventional action for a neurotransmitter, recent experiments suggest that this does occur in cells of the frog sympathetic ganglion³⁰. Alternatively it is possible that depolarizing bipolar cells do not receive direct synaptic input from receptors, a suggestion that has already been made by Tomita⁸.

The second question concerns the identification of the photoreceptor neurotransmitter. It has been shown that certain amino-acids rapidly depolarize skate horizontal cells^{17,31,32}. L-Na aspartate is especially potent in this respect (see Fig. 2 in Dowling and Ripps¹⁷), and might be considered as a possible candidate for the photoreceptor transmitter. It is noteworthy that recent work indicates that L-glutamate is the probable neurotransmitter at electroreceptor synapses in the skate³³.

This research was supported by grants from the National Eye Institute, US Public Health Service, and from Fight for Sight, Inc., New York City.

Received October 30, 1972.

- ¹ DeRobertis, E., and Franchi, C. M., *J. Biophys. Biochem. Cytol.*, **2**, 307 (1956).
- ² Missotten, L., *The Ultrastructure of the Retina* (Arscia Uitgaven, Brussels, 1965).
- ³ Gray, E. G., and Pease, H. L., *Brain Res.*, **35**, 1 (1971).
- ⁴ Werblin, F. S., and Dowling, J. E., *J. Neurophysiol.*, **32**, 339 (1969).
- ⁵ Trifonov, Y. A., and Byzov, A. L., *Biophysica*, **10**, 673 (1965).
- ⁶ Byzov, A. L., and Trifonov, J. A., *Vision Res.*, **8**, 817 (1968).
- ⁷ Trifonov, Y. A., *Biofizika*, **13**, 5 (1968).
- ⁸ Tomita, T., *Quart. Rev. Biophys.*, **3**, 179 (1970).
- ⁹ Svaetichin, G., and MacNichol, E. F., *Ann. NY Acad. Sci.*, **74**, 385 (1958).
- ¹⁰ Dowling, J. E., and Ripps, H., *J. Gen. Physiol.*, **58**, 163 (1971).
- ¹¹ Toyoda, J., Nosaki, H., and Tomita, T., *Vision Res.*, **9**, 453 (1969).
- ¹² Hagins, W. A., Penn, R. D., and Yoshikami, S., *Biophys. J.*, **10**, 380 (1970).
- ¹³ Del Castillo, J., and Engbaek, K., *J. Physiol.*, **124**, 370 (1954).
- ¹⁴ Takeuchi, A., and Takeuchi, N., *J. Gen. Physiol.*, **45**, 1181 (1962).
- ¹⁵ Katz, F., and Miledi, R., *J. Physiol.*, **192**, 407 (1967).
- ¹⁶ Dowling, J. E., and Ripps, H., *J. Gen. Physiol.*, **56**, 491 (1970).
- ¹⁷ Dowling, J. E., and Ripps, H., *J. Gen. Physiol.*, **60**, 698 (1972).
- ¹⁸ *Formulae and Methods Manual*, **5**, 56 (Marine Biological Laboratory, Woods Hole, Massachusetts, 1956).
- ¹⁹ Del Castillo, J., and Katz, B., *J. Physiol.*, **124**, 553 (1954).
- ²⁰ Hubbard, J. I., *J. Physiol.*, **159**, 507 (1961).
- ²¹ Fatehchand, R., Svaetichin, G., Negishi, K., and Drujan, B., *Vision Res.*, **6**, 271 (1966).
- ²² Negishi, K., and Svaetichin, G., *Pflügers Archiv.*, **292**, 177 (1966).
- ²³ Winkler, B. S., *Vision Res.*, **12**, 1183 (1972).
- ²⁴ Brown, K. T., *Vision Res.*, **8**, 633 (1968).
- ²⁵ Noell, W. K., *USAF School of Aviation Med. Project No. 21-1201-0004, Report 1* (1954).
- ²⁶ Schmidt, R., and Steinberg, R., *J. Physiol.*, **217**, 71 (1971).
- ²⁷ Miller, R. F., and Dowling, J. E., *J. Neurophysiol.*, **33**, 323 (1970).
- ²⁸ Sillman, A. J., Ito, H., and Tomita, T., *Vision Res.*, **9**, 1435 (1969).
- ²⁹ Nelson, R., thesis, Johns Hopkins Univ. (1972).
- ³⁰ Weight, F. F., *Soc. Gen. Physiol. Symp.* (in the press).
- ³¹ Dowling, J. E., and Ripps, H., *Biol. Bull.*, **141**, 384 (1971).
- ³² Cervetto, L., and MacNichol, jun., E. F., *Biol. Bull.*, **141**, 381 (1971).
- ³³ Steinbach, A. B., *Soc. Gen. Physiol. Symp.* (in the press).

LETTERS TO NATURE

PHYSICAL SCIENCES

Tracks from Extinct Radioactivity, Ancient Cosmic Rays, and Calibration Ions

WE present here an alternative explanation of the fossil tracks in lunar minerals that have been attributed^{1,2} to extinct radioactivity of superheavy elements.

Particle track etching of crystals from extraterrestrial materials has yielded a wealth of information about cosmic rays and spontaneously fissioning nuclides, and consequently about nuclear synthesis, early Solar System history, and the origin and history of cosmic rays. The lengths of tracks from cosmic ray nuclei that penetrate meteorites can be used to measure the abundances of heavy cosmic rays^{3,4} and these tracks can be distinguished by various features from tracks caused by fission^{3,5,6}. Simple means of measuring the full track lengths have been developed by Lal *et al.*⁷ and used to infer detailed cosmic ray abundances.

In applying this technique to lunar and meteoritic samples Bhandari *et al.*^{1,2} assumed (reasonably) that the most abundant tracks (those of length 11 to 12 μm) were from Fe nuclei. Uranium-235 fission tracks were shown to have lengths ~ 14 to 15 μm and the observed excess of natural tracks of that length over what could be expected from the uranium concentration present was attributed^{1,2} to the past decay of now extinct ²⁴⁴Pu, a nuclide which existed early in the history of the Solar System^{5,6,8}. Tracks of $\sim 25 \mu\text{m}$ length then became an anomaly, which Bhandari *et al.*^{1,2} attributed to the fission of now extinct superheavy nuclei (nuclear charge ~ 100 –114, atomic mass ~ 300). Such nuclei would be expected to produce unusually long tracks⁹.

The positive identification of superheavy elements, if confirmed, would be a remarkable culmination of a sequence of searches by many workers, using a variety of techniques that have yielded no positive results which have survived careful examination. But it is a far from trivial task to identify in this manner a minority segment of the fission, which is itself a small component within a large cosmic ray background. In particular the first attempts to infer the length of fresh tracks of Fe nuclei using accelerator ions indicated a probable length¹⁰ that was greater than that observed for the most abundant natural tracks in extraterrestrial crystals.

More direct, recent results^{11,11a} have shown a clear discrepancy to exist. In the first of these works a beam of Fe ions of 10 MeV/nucleon allowed the full, etchable track lengths to be measured: the distribution is peaked at 19 to 21 μm cm with a bell shaped distribution in lengths ranging from 12 to 24 μm cm in lunar pyroxene and meteoritic enstatite. In the second of these works fresh, natural iron-group tracks in a feldspar crystal that had been exposed to solar cosmic rays on the Apollo 16 mission were found to extend up to 30 μm in length. In contrast the lunar tracks were most abundant at significantly shorter lengths (13 or 15 μm) and were even less sharply peaked than was the distribution of calibration tracks.

Either the natural tracks that were previously inferred to be Fe are in fact lighter nuclei, or some mechanism has shortened

the Fe tracks. In principle the more abundant tracks could be the lighter, spallation products of iron produced by nuclear interactions within the meteorite or lunar sample. Because the interaction mean free path for iron is $\sim 6 \text{ cm}$ of rock³, this alternative would require that the 11 to 12 μm peaks would always have been seen in samples from depths appreciably greater than that; that is not the case. In addition, direct experiments¹² as a function of depth at lesser depths show the 11 to 12 μm peak.

We turn therefore to the alternative that the most abundant tracks are Fe, but that they have been altered over time. Two mechanisms are known for track alteration: heating¹³ and deformation¹⁴. The first is far more extensively documented. Based on the extrapolations of laboratory annealing experiments, tracks in most meteoritic and lunar crystals exposed to the expected temperatures should be stable and unaltered^{3,11,13,15} throughout their cosmic ray exposure times. If the usual exponential form of annealing kinetics were to break down by the intervention of a second mechanism with a lower activation energy, it would be possible for annealing to occur. Such a behaviour has, for example, been observed on one occasion for argon diffusion in mica¹⁶. But such a result has never been obtained for track annealing in the many cases where the kinetics have been measured¹⁷; in short there is no evidence for such an effect.

In contrast there is evidence for mechanical effects. Most samples from the lunar surface have been exposed to shock—the agency that broke them up from larger rocks. Similarly meteorites have been bombarded in space and end their cosmic ray exposure by experiencing high pressures as they encounter the Earth's atmosphere; finally, the larger surviving fragments are shocked by impact with the ground. We have observed track shortening for cosmic ray tracks in deformed lunar pyroxenes¹⁴ and track fading and erasure for uranium fission fragment tracks both in individual minerals shocked in the laboratory¹⁸ and in natural rocks that were exposed to shock conditions (unpublished). We have now measured fission fragment track lengths as a function of shock pressure for apatite taken from granodiorite shocked to pressures of 11, 13, 20 and 30 kbar (unpublished). We find that the median track lengths on a polished section are 5.5, 5.4, 2.7 and 1.5 μm at the four pressures. The corresponding maximum track lengths on a polished section are 9.5, 12.0, 8.8 and 6.5 μm . (In contrast biotite shocked normal to the layer planes shows less than a 10% shortening up to 92 kbar.) Of these two results the minerals from shocked rock are the more directly pertinent, since they more closely simulate the shock conditions to be expected in lunar and meteoritic samples.

To summarize, track lengths are decreased in a manner consistent with the shift of most observed natural cosmic ray tracks in crystals^{1,2,11,12} relative to the calibration Fe tracks^{11,11a}. If this explanation is tentatively accepted, then the longer tracks observed by Bhandari *et al.*^{1,2} and attributed to fission of ²⁴⁴Pu and superheavy nuclei would be explained most directly as normal, unaltered cosmic ray Fe or Ni tracks that have been formed since the last shock event that produced shortening of tracks. Further and more direct evidence of shock of the particular pyroxenes used by Bhandari *et al.*^{1,2} might be obtainable by examination, using transmission elec-

tron microscope replicas, of their samples etched with the acid etch that has been shown to reveal deformation markings¹⁹.

We thank W. R. Giard for experimental assistance. This work was supported by NASA.

R. L. FLEISCHER
H. R. HART, jun.

General Electric Research and
Development Center,
Schenectady, New York 12301

Received January 25, 1973.

- ¹ Bhandari, N., Bhat, S. G., Lal, D., Rajagopalan, G., Tamhane, A. S., and Venkatavaradan, V. S., *Nature*, **230**, 219 (1971).
- ² Bhandari, N., Bhat, S. G., Lal, D., Rajagopalan, G., Tamhane, A. S., and Venkatavaradan, V. S., *Second Lunar Sci. Conf., Geochim. Cosmochim. Acta Suppl.* **2**, 3, 2599 (1971).
- ³ Fleischer, R. L., Price, P. B., Walker, R. M., and Maurette, M., *J. Geophys. Res.*, **72**, 331 (1967).
- ⁴ Fleischer, R. L., Price, P. B., Walker, R. M., Maurette, M., and Morgan, G., *J. Geophys. Res.*, **72**, 355 (1967).
- ⁵ Fleischer, R. L., Price, P. B., and Walker, R. M., *J. Geophys. Res.*, **70**, 2703 (1965).
- ⁶ Fleischer, R. L., Price, P. B., and Walker, R. M., *Geochim. Cosmochim. Acta*, **32**, 21 (1968).
- ⁷ Lal, D., Rajan, R. S., and Tamhane, A. S., *Nature*, **221**, 33 (1969).
- ⁸ Rowe, M. W., and Kuroda, P. K., *J. Geophys. Res.*, **70**, 709 (1965).
- ⁹ Price, P. B., and Fleischer, R. L., *Phys. Lett.*, **30**, B, 246 (1969).
- ¹⁰ Price, P. B., Fleischer, R. L., and Moke, C. D., *Phys. Rev.*, **167**, 277 (1968).
- ¹¹ Plieninger, T., and Krätschmer, W., *Proc. Eighth Int. Conf. on Nucl. Photog. and Solid State Detectors* (Bucharest, 1972).
- ¹² Burnett, D. S., Maurette, M., Monnin, M., Walker, R. M., and Woolum, D., *Apollo 16 Preliminary Science Report, NASA* (1973).
- ¹³ Bhandari, N., Bhat, S. G., Goswami, J. N., Lal, D., Tamhane, A. S., and Venkatavaradan, V. S., *Lunar Science III* (edit. by Watkins, C.), 65 (Lunar Sci. Inst. Publ. 88, 1972).
- ¹⁴ Fleischer, R. L., Price, P. B., and Walker, R. M., *J. Geophys. Res.*, **70**, 1497 (1965).
- ¹⁵ Fleischer, R. L., Comstock, G. M., and Hart, jun., H. R., *J. Geophys. Res.*, **77**, 5050 (1972).
- ¹⁶ Crozaz, G., Haack, U., Hair, M., Maurette, M., Walker, R. M., and Woolum, D., *Apollo 11 Lunar Sci. Conf., Geochim. Cosmochim. Acta Suppl.* **1**, 3, 2051 (1970).
- ¹⁷ Reynolds, J. H., *Geochim. Cosmochim. Acta*, **12**, 177 (1957).
- ¹⁸ Fleischer, R. L., and Hart, jun., H. R., *Proc. Conf. on Calibration of Hominoid Evolution* (edit. by Bishop, W. W., Miller, J. A., and Cole, S.), 135 (Scottish Academic Press, Edinburgh, 1972).
- ¹⁹ Ahrens, T. J., Fleischer, R. L., Price, P. B., and Woods, R. T., *Earth. Planet. Sci. Lett.*, **8**, 420 (1970).
- ²⁰ Fleischer, R. L., and Hart, jun., H. R., *J. Geophys. Res.* (in the press).

Radio Pulses from the Direction of the Galactic Centre

WEBER¹⁻³ has reported detecting pulses of gravitational radiation. The flux density per event is about 3×10^5 erg cm⁻² s⁻¹ Hz⁻¹ at 1,660 Hz, implying a possible release of 10^{54} erg, equivalent to about $1 M_{\odot}$, if the radiation originates in the region of the Galactic centre. It has been suggested that if only 1 part in 10^{25} of the energy were radiated in the electromagnetic spectrum, this could be detected. Some attempts have been made at radio wavelengths⁴⁻⁶, at optical wavelengths (J. T. Delaney, B. Lawless and G. A. Baird, unpublished) and in the X-ray band^{7,8}, but so far without success. Here we describe the results of a new experiment to correlate radio pulses at 858 MHz with events observed by Weber, but to a better radio sensitivity than has been achieved so far. The results are inconclusive at present, but there is strong evidence for the existence of discrete radio pulses of extra-terrestrial origin.

We used a 60-foot telescope operating at 858 MHz. To reduce spurious instrumental effects, we used a receiver of the correlation type in which the signal from the antenna is split between two identical channels, the outputs being multiplied together and then displayed on an analogue recorder. The

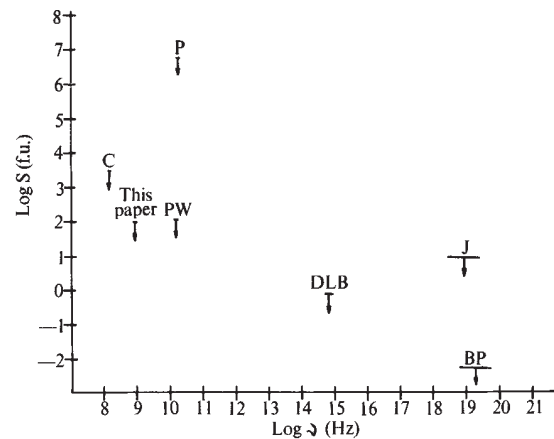


Fig. 1 Pulse detection limits achieved by various observers. C, Charman *et al.*⁴; P, Partridge⁵; PW, Partridge and Wrixon⁶; DLB, Delaney, Lawless and Baird (unpublished); J, Jelley⁷; BP, Baird and Pomerantz⁸.

effective time constant at the output was about 1 s. Overall sensitivity was determined from observation of the Crab Nebula, assumed to have a flux density of 1,062 f.u. at 858 MHz. The r.m.s. noise level at the output, σ_N , corresponded to a signal received in the direction of the antenna maximum of 14 f.u. The half-power beamwidth of the antenna was 1.4 arc deg and approximated in angular size the extended regions of radio, X-ray and infrared emission at the Galactic centre. The minimum detectable level for pulses was $6\sigma_N$ at the recorder, corresponding to a received signal of 85 f.u. This sensitivity is compared with that of previous observers in Fig. 1.

Because the amplitude of possible pulses is not known the direction of origin of the source could be inside a cone with dimensions many times that of the beamwidth of the telescope. To improve angular resolution, we took a series of drift scans at constant declination, and attempted to correlate positional distribution of the pulses with the beamwidth. The area of sky scanned was approximately 1.4×4.4 arc deg², centred on Sgr A. Each scan took 20 min, and up to eleven such scans could be obtained each day.

A radio event was defined as a pulse which occurred during a useful observing session—one that was interference free and showed no receiver drifts, with pulse amplitude at least equal to six times the r.m.s. output fluctuation, and pulse duration not much greater than the recorder time-constant of 1 s. Fig. 2 shows an example of such an isolated event; the increase in level due to radiation from the region of the Galactic centre is also seen. Local interference and lightning discharges were observed on a number of occasions and necessitated the rejection of approximately 40% of the total data.

During June to December 1971, 804 useful drift scans were obtained giving a total of 270 h of observation. 97 events were noted, a mean reception rate of 0.36 events h⁻¹. The maximum amplitude was $30\sigma_N$, and the smallest recorded was determined from the criterion of detectability, $6\sigma_N$. The mean

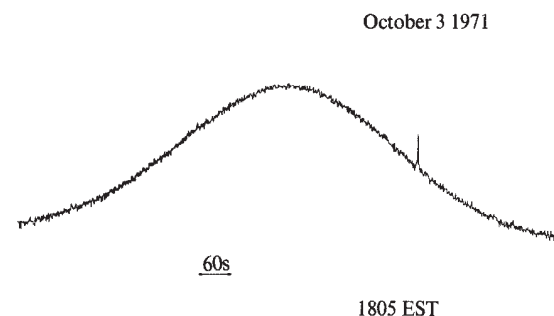


Fig. 2 Example of isolated pulse observed during drift scan of Galactic centre.

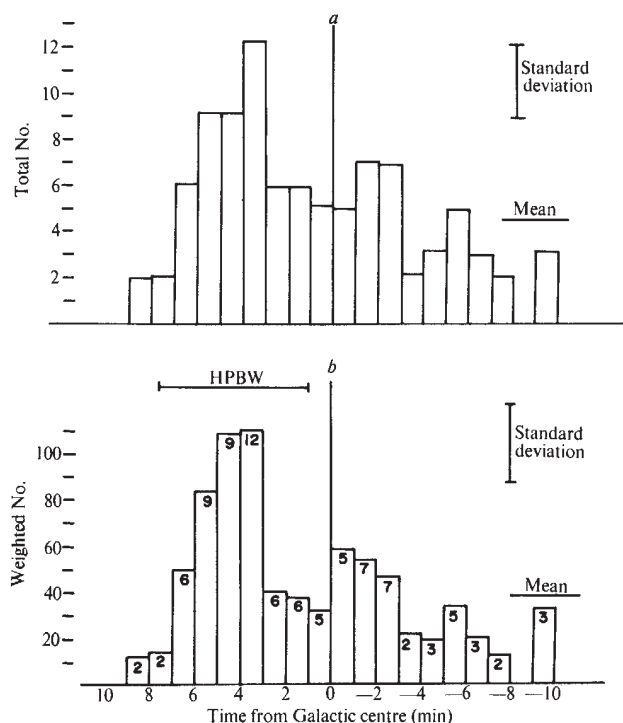


Fig. 3 Normalized distribution of radio pulses against time relative to continuum peak (T). *a*, Number of pulses; *b*, number of pulses weighted according to pulse amplitude.

amplitude was $9.7\sigma_N$ with a standard deviation $4.8\sigma_N$. As an initial check that the events were not due to weak interference from fixed terrestrial sources, the distribution with azimuth pointing of the antenna was first determined. A small increase in the distribution around azimuth 200° was noted, but was not statistically significant. We concluded that there was no significant dependence of event rate with azimuth.

For the first stage of the analysis, we attempted a correlation of radio events with the data on gravitational events supplied by Weber, though the latter were not complete for the period of our observations. But 222 gravitational events were recorded on 96 of the days, corresponding to a rate of 0.096 events h^{-1} . The number occurring during useful radio observing periods was 14; the expected number was 18.4. We examined the radio records at the times of the Weber events, but observed no radio feature of any kind within a period of ± 5 min.

An analysis of the time interval between the radio and gravitational events was carried out for all events. Of the 21,534 time intervals, 103 occurred within the range $\pm 1,000$ min, and 9 occurred within ± 60 min, in general agreement with the numbers expected if the events occurred independently and at random. The minimum time interval was 10.1 min and no significant grouping at any time interval was found. The expected delay of the radio events is 6 s, assuming that the gravitational waves travel undispersed with velocity c and the dispersion measure for radio waves propagating from the Galactic centre is $1,000$ pc cm^{-3} . We therefore concluded that there was no significant correlation between gravitational events and the observed radio events. But coincidences could have been missed, because any one region of the Galactic centre was observed for only a fraction of the total time. The probability of a coincidence depends chiefly on the length of scan, the threshold level for detection and the amplitude probability distribution of the pulses. The latter is not known, but for pulses of amplitude $9.7\sigma_N$, on average only one in 7.5 would be observed; for those of amplitude $14.5\sigma_N$, corresponding to one standard deviation greater than the mean, the probability is one in 5.5. Also, for about a half-beamwidth at the ends of the scans (3.2 min), the probability is less by a factor of

about 2. Of the 14 gravitational events, an average of only $2 (\pm 2)$ coincidences would be expected. So the results are not statistically significant, and in addition, the source of gravitational events could well lie outside the radio search area.

We also tried to determine the direction of origin of the events by measuring their time of occurrence relative to the peak of the continuum radiation from the Galactic centre (Fig. 2a). Fig. 3b shows a similar distribution, with the individual events weighted according to pulse amplitude; the numbers in the latter distribution refer to the number of events.

There is a slight trend towards an increase in number received from the direction of the Galactic centre (Fig. 3a), and an increase at about 3.5 min after transit. But if we take into account the amplitude of the pulses (Fig. 3b), it seems that there is a significant increase between $+3$ and $+6$ min. The peak corresponds to an increase of 2.4 s.d. over the mean, the probability of this occurring by chance being about 2%. Because the adjacent bin is of comparable amplitude, the probability of the two occurring together by chance is less than 0.1%. A change in the width of the bins does not seriously affect the statistics, but if the three bins between $+3$ and $+6$ min are not used in the determination of the mean and s.d., the significance of the amplitude of the largest is about five times the new s.d. There seems to be little difference in the two distributions, apart from the increase in the weighted number histogram, so we conclude that there might be a discrete source which is giving consistently larger pulses than average, for the region scanned.

We estimate the coordinates of this source to be:

$$RA = 17^h 48.2 \text{ min} \pm 1.3 \text{ min} \quad Dec = -28^\circ 58' \pm 52' (1950.0)$$

and the coordinates of the peak in the continuum emission are:

$$RA = 17^h 43.8 \text{ min} \pm 0.8 \text{ min} \quad Dec = -28^\circ 58' \pm 10' (1950.0)$$

The half-power beamwidth of the telescope seems to be larger in RA than the extent of the source (Fig. 3b). But this is consistent with a cutoff applied to the amplitude of the events. For a mean amplitude of pulses from the source of $9.7\sigma_N$, the apparent half-width of the source would be reduced from 6.4 min in RA to 3.5 min.

Some observations were also made in the direction of other sources and also at the same azimuths as those used for Galactic centre observations, though not for the same length of time. In 22 h observing, only one pulse occurred. We conclude that in the direction of the Galactic centre there are sources which emit pulses of radio waves of duration less than 1 s (as determined by the output time constant of the recording equipment) and a source which seems to contribute about 24% of the events, with pulses of consistently greater amplitude than the average of all events.

How do these events relate to known radio, optical, infrared, X-ray and γ -ray sources? Fig. 4 shows the scan region superimposed on a 5 GHz radio contour map⁹, with the positions of 100 μ m infrared sources¹⁰, Uhuru sources¹¹, and PSR 1749-28 (ref. 12). The latter, within 0.35 ± 0.29 arc deg in RA and 0.87 arc deg in declination from the possible discrete source, could be generating super pulses. If so, they would be of amplitude $\sim 10\sigma$ which, with the recorder time constant of 1 s and correction for the position of the pulsar in the telescope beam, gives an individual pulse energy of 4.0×10^{-21} erg cm^{-2} Hz^{-1} . The mean energy spectrum for the Crab pulsar, which has similar dispersion measure and may be at a similar distance, is¹³ $\int I dt = 4 \times 10^{-22} (v/100)^{-1.9}$ erg cm^{-2} Hz^{-1} , a factor of about 600 less than the intensity of the observed pulses. But at 146 MHz giant individual pulses from the Crab of energy 600 times greater than the mean are observed at a rate of about one per day¹⁴. The estimated pulse rate from the source is about $0.5 h^{-1}$, when corrections for the method of observation have been applied. But because of the small amount of information available on super pulses from other pulsars, a direct comparison is probably not significant. It is likely that PSR 1749-28 (equivalent age $P/\dot{P} = 2 \times 10^6$ yr) is considerably older than the Crab pulsar (2.5×10^3 yr, ref. 15).

Further, though the dispersion measures for both pulsars have similar values, it is likely that the mean interstellar electron density is appreciably greater in directions towards the Galactic centre than in directions towards the anti-centre. Cavallo and Ventura¹⁶ give a distance of 366 pc for PSR 1749-28 in comparison with 1,527 pc for the Crab from an analysis of their periodicity-luminosity relationship. This implies that any giant pulses produced by PSR 1749-28 are at least an order of magnitude smaller than those produced by the Crab pulsar.

The possibility remains that most of the other events could also be produced by pulsars and hence could be related to supernova remnants in the Galactic centre region. The Crab pulsar was first detected from observations of giant pulses and is one of the few associations of a pulsar with a supernova remnant. Of the nearly 80 pulsars known at present, the only other two associations are in the constellations Vela¹⁷ and Crux¹⁸ and with IC 443 (ref. 18a). A number of supernova remnants exist in the direction of the Galactic centre (Fig. 4 and refs. 19, 20).

We find no correlations between the times of occurrence of the radio events and those of solar radio bursts, solar flares and sudden ionospheric disturbances.

Some of the radio events may be due to electromagnetic radiation from extensive air showers produced by cosmic rays (refs. 21, 22). The intensity at high frequencies of such a shower is proportional to $\cos^6 Z$ where Z is the zenith angle²³, and has the rather steep spectral index of 2. Because the maximum radio flux for showers occurring at the zenith at 520 MHz is about 10^5 f.u. for a minimum zenith angle at 74° the expected amplitude at 858 MHz is 16 f.u., less than 20% of the detectable level. The more recent results by Charman²⁴ at 500 MHz confirm this estimate.

Other interesting speculations are that the events might be associated with black holes, and produced either by charged particles falling into them^{25,26} (which might also produce detectable gravitational radiation) or by the collision of black holes²⁷. The discrete pulse source might then be produced by the former, but would require massive clouds of particles to fall into the black hole at the rate of about one every 10 h. The other events might also be produced in a similar way, but the collision mechanism, which would require one collision about every three hours, needs a rather large supply of black holes at the Galactic centre.

We thank Professor J. Weber for providing data on gravitational events, Mr Bob Baran for assistance in the construction

and operation of the equipment and Dr P. Feldman for discussions. The work was carried out under an operating grant from the National Research Council of Canada.

V. A. HUGHES
D. S. RETALLACK

Astronomy Group,
Department of Physics,
Queen's University at Kingston, Ontario

Received October 2, 1972.

- ¹ Weber, J., *Phys. Rev. Lett.*, **22**, 1320 (1969).
- ² Weber, J., *Phys. Rev. Lett.*, **25**, 180 (1970).
- ³ Weber, J., *Nuovo Cim.*, **4**, B, 197 (1971).
- ⁴ Charman, W. N., Fruin, J. H., Jelley, J. V., Haynes, R. F., Hodgson, E. R., Scott, P. F., Shakeshaft, J. R., Baird, G. A., Delaney, T. J., Lawless, B. G., Drever, R. W. P., and Meikle, W. P. S., *Nature*, **232**, 177 (1971).
- ⁵ Partridge, R. B., *Phys. Rev. Lett.*, **26**, 912 (1971).
- ⁶ Partridge, R. B., and Wrixon, G. T., *Astrophys. J. Lett.*, **173**, L75 (1972).
- ⁷ Jelley, J. V., *Nature*, **234**, 142 (1971).
- ⁸ Baird, G. A., and Pomerantz, M. A., *Phys. Rev. Lett.*, **28**, 1337 (1972).
- ⁹ Altenhoff, W. J., Downes, D., Goad, L., Maxwell, A., and Rinehart, R., *Astron. Astrophys. Suppl.*, **1**, 319 (1970).
- ¹⁰ Hoffmann, W. F., Frederick, C. L., and Emery, R. J., *Astrophys. J. Lett.*, **170**, L89 (1971).
- ¹¹ Giacconi, R., Murray, S., Gursky, H., Schreier, E., Tananbaum, H., and Kellogg, E., *Astrophys. J.* (in the press).
- ¹² Turtle, A. J., and Vaughan, A. E., *Nature*, **219**, 689 (1968).
- ¹³ Heiles, C., and Rankin, J. M., *Nature Physical Science*, **231**, 97 (1971).
- ¹⁴ Argyle, E., and Gower, J. F. R., *Astrophys. J. Lett.*, **175**, L89 (1972).
- ¹⁵ Manchester, R. N., and Taylor, J. H., *Astrophys. Lett.*, **10**, 67 (1972).
- ¹⁶ Cavallo, G., and Ventura, A., *Astron. Astrophys.*, **18**, 287 (1972).
- ¹⁷ Large, M. I., Vaughan, A. E., and Mills, B. V., *Nature*, **220**, 340 (1968).
- ¹⁸ Large, M. I., and Vaughan, A. E., *Nature Physical Science*, **236**, 117 (1972).
- ^{18a} Davies, J. G., Line, A. G., and Seiradakis, J. H., *Nature*, **240**, 229 (1972).
- ¹⁹ Downes, D., *Astron. J.*, **76**, 305 (1971).
- ²⁰ Ilovaisky, S. A., and Lequeux, J., *Astron. Astrophys.*, **18**, 169 (1972).
- ²¹ Allan, H. R., Neat, K. P., and Jones, J. K., *Nature*, **215**, 267 (1967).
- ²² Lerche, I., *Nature*, **215**, 268 (1967).
- ²³ Fegan, D. J., and Slevin, P. J., *Nature*, **217**, 440 (1968).
- ²⁴ Charman, W. N., *Lett. Nuovo Cim.*, **3**, 845 (1970).
- ²⁵ Ross, D. K., *Publ. Astron. Soc. Pacific*, **83**, 633 (1971).
- ²⁶ Ruffini, R., and Wheeler, J. A., in *The Significance of Space Research for Fundamental Physics* (edit. by Moore, A. F., and Hardy, V.), 45 (ESRO, 1971).
- ²⁷ Ross, D. K., *Nuovo Cim.*, **B**, **6**, 83 (1971).

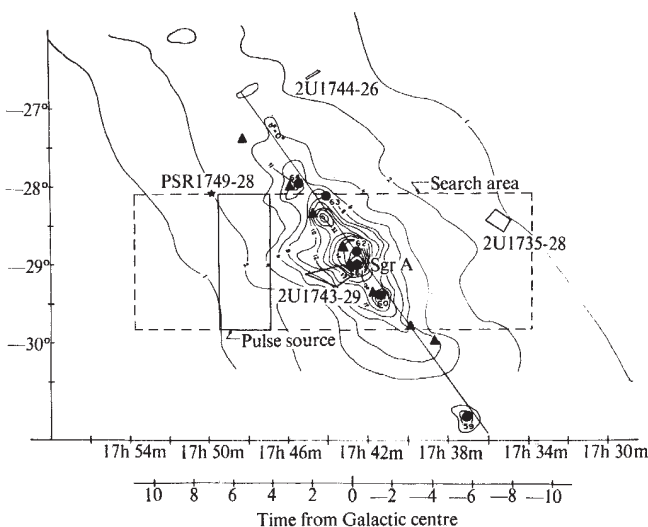


Fig. 4 Search area superimposed on 5 GHz radio contour map⁹. Also shown are Uhuru sources¹¹, PSR 1749-28 (ref. 12), 100 μ m infrared sources ($\blacktriangle$) (ref. 10) and non-thermal radio sources ($\bullet$) (ref. 20).

5- μ m Infrared Emission from Algol

DURING September 1972, we made a number of 5- μ m observations of the eclipsing binary β Per (Algol) using the 60-inch infrared flux collector at Izana, Tenerife. Our photometer was of conventional design, having a nodding mirror to give sky background cancellation and using an indium antimonide detector cooled to 77 K. α Per was used as a comparison source and this in turn was calibrated against better known infrared standards such as α Lyr. Poor weather conditions did not allow us to make as many observations as we would have liked, but sufficient to define the principal light curve.

Fig. 1 shows our experimental results: the infrared light curve has been drawn on the assumption that it is symmetrical about both the primary and secondary minima. The principal source of error was vibration of the telescope in high winds. The Algol system is thought¹ to consist of three components: A—a main-sequence B8 star ($M_{bol} = -0.4$, $\log T_e = 4.03$); B—a KO subgiant ($M_{bol} = +3.1$, $\log T_e = 3.66$); C—a main-sequence A5 star ($M_{bol} = +2.3$, $\log T_e = 3.92$). This gives a total apparent M magnitude of +2.86.

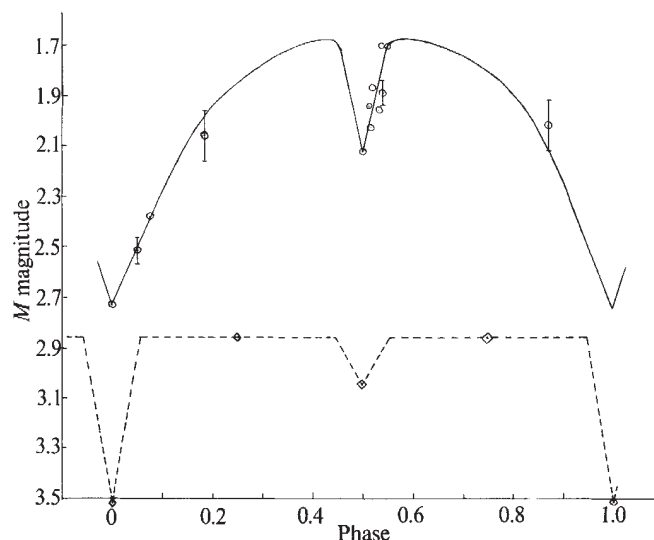


Fig. 1 Observations of Algol at $5\ \mu\text{m}$. —, Infrared light curve; ---, total intensity of the system, assuming that the component stars behave like black bodies.

There is clearly an infrared excess of 1.2 mag. The shape of the infrared light curve indicates that this infrared emission cannot be from the stars A and B; if it were, the infrared light curve would have the same shape as the optical light curve (dotted curve).

Chen² has observed Algol at $1.6\ \mu\text{m}$ and found that the form of the infrared variation at that wavelength is similar to the optical variation. Thus whatever is causing the infrared excess at $5\ \mu\text{m}$ should not cause an appreciable excess at $1.6\ \mu\text{m}$. This rules out an explanation in terms of a dust shell around the two stars, because the dust must have a temperature of less than 600 K for it not to radiate at $1.6\ \mu\text{m}$. This, in turn, suggests that the dust must be at distances greater than $300\ R_{\odot}$ from star A. A and B are separated by only $15.7\ R_{\odot}$, so that a dust shell of radius $300\ R_{\odot}$ or more could not provide any form of infrared eclipse.

The infrared excess can, however, be explained by free-free emission from an ionized hydrogen plasma cloud. Because we have observations at only one wavelength, we cannot provide a definitive model. A cloud of radius $15\ R_{\odot}$, temperature 10,000 K and electron density $10^9\ \text{cm}^{-3}$ would radiate the required $5\ \mu\text{m}$ flux. Such a cloud would be optically thin at infrared and visible wavelengths, but would become optically thick at about $\lambda = 5\ \text{mm}$. It would not emit detectable fluxes of either radio or visible continuum radiation, nor would it emit detectable line emission (for example, H_{α}). If the cloud has a temperature of 100,000 K the electron density can be slightly less, and the cloud does not become optically thick until $\lambda = 1.5\ \text{cm}$.

Algol is a radio flare star whose spectrum rises at shorter wavelengths^{3,4}. It has been observed at wavelengths of 11.1 cm, 3.7 cm and 2.8 cm, agreeing reasonably with the optical density we have estimated for the cloud at radio wavelengths. (The radio emission—thought to be synchrotron in origin—may have to pass through at least some of the cloud.)

Because the cloud must be optically thin at $5\ \mu\text{m}$, only stars A and B can produce the eclipses. The infrared excesses at primary minimum, secondary minimum and at maximum are in the ratios 1:2:4.5. It seems difficult to explain these figures if the gas cloud fills the Roche lobes, or orbits either or both of the stars. We conclude that most of the cloud must lie between the two stars. Thus the cloud will be smaller and slightly denser than we have suggested above. Finally, we emphasize that the curve shown is the best fit to our data but for any eclipsing system the intensity maximum would be expected at 0.25 phase.

We acknowledge the support of the SRC towards equipment and travel expenses to Tenerife. One of us (A. J. L.) is the holder of an SRC research studentship.

R. F. JAMESON
A. J. LONGMORE
B. CRAWFORD

Department of Astronomy,
Leicester University

Received January 24, 1973.

- ¹ Hill, G., Barnes, J. V., Hutchings, J. B., and Pearce, J. A., *Astrophys. J.*, **168**, 443 (1971).
- ² Chen, K. Y., and Renning, E. G., *Astron. J.*, **71**, 283 (1966).
- ³ Hughes, B. A., and Woodsworth, A., *Nature Physical Science*, **236**, 42 (1972).
- ⁴ Hjellming, R. M., and Webster, E., *Nature Physical Science*, **236**, 43 (1972).

Radio Source Counts in Cosmology

SCHMIDT¹ has objected to an argument given in my 1968 Bakerian Lecture². Fig. 2 of Kellermann's recent Warner Prize Lecture³ shows combined differential source counts at 75, 20, 11 and 6 cm. The points show a Euclidean distribution for values of the integral count between ~ 10 and $\sim 10^3$ sources sr^{-1} . Above 10^3 sources sr^{-1} the counts deviate from a Euclidean distribution in the way that is expected for cosmologically distant objects, but for less than 10 sources sr^{-1} the counts deviate from Euclidean in a reversed sense. This so-called "steep" part of the distribution therefore involves only ~ 100 sources over the whole sky. The point from my Bakerian Lecture, now under criticism, was that the latter deviation can be regarded as a local fluctuation in which ~ 5 nearby sources sr^{-1} are considered to be missing at the high flux end of the source distribution. The data from four surveys, given in the reference cited above, strongly support this position.

The Euclidean behaviour for source counts between 10 and $10^3\ \text{sr}^{-1}$ can be understood on the basis that the sources in question are not at significant cosmological distances. This explanation can be used either for the steady state model or for non-evolving Friedmann models. The opposite point of

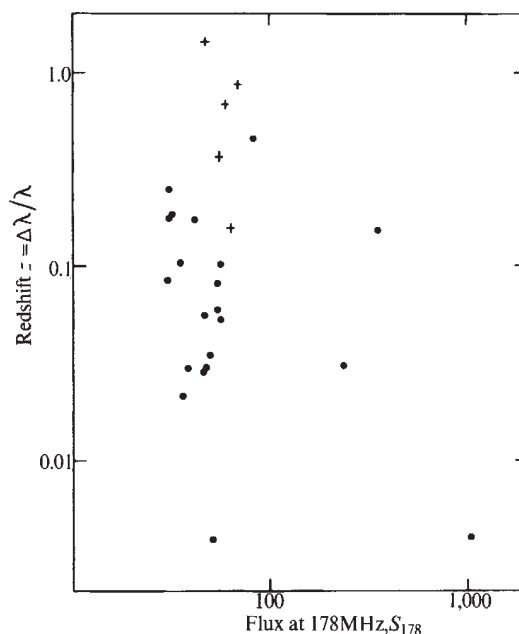


Fig. 1 Plot of redshift z against flux at 178 MHz, S_{178} , for the 26 sources of the 3 CR catalogue with $S_{178} > 30 \times 10^{-26}\ \text{W m}^{-2}\ \text{Hz}^{-1}$. +, QSO; ●, radio galaxy.

view, that the distances are of cosmological order, leads to the first of the four curious coincidences discussed by Kellermann³.

By adding optical data it would be possible to carry the argument further. Redshifts are not available, however, for more than a small fraction of the sources. The data discussed by Schmidt concern only 26 very high-flux sources. The redshifts z of these sources, plotted against the 178 MHz flux S_{178} , are shown in Fig. 1. The absence of a significant correlation between redshifts and fluxes can be interpreted in two ways: the optical redshifts are not distance indicators, or the 26 sources in question constitute a fluctuation.

In the first case, issues striking at the root of our most cherished cosmological beliefs would be involved. In the second case it is meaningless to attempt to base far reaching cosmological conclusions on a very small sample of 26 sources constituting only a fluctuation.

This work was supported in part by the National Science Foundation.

F. HOYLE

California Institute of Technology,
Pasadena, California

Received December 4, 1972.

¹ Schmidt, M., *Nature*, **240**, 399 (1972).

² Hoyle, F., *Proc. Roy. Soc., A*, **308**, 1 (1969).

³ Kellermann, K. I., *Astron. J.*, **77**, 531 (1972).

Reply to Hoyle

HOYLE's assertion that only 5 sources per steradian are missing at the bright end¹ is based on the apparent Euclidean nature of the source counts. My derivation of the steady-state source counts² expected if radio galaxies have cosmological redshifts shows that the Euclidean slope of -1.5 is not reproduced at the count levels of interest.

The large scatter in the diagram of redshift versus radio flux density shown by Hoyle¹ is usually interpreted as caused by the large range of absolute radio luminosities—an interpretation not mentioned by Hoyle. The tight correlation between the redshift and the optical flux density of radio galaxies³ is quite similar to that observed for the brightest galaxy in clusters of galaxies⁴. There is at present no attractive alternative to the conclusion (or, in Hoyle's words, the cherished cosmological belief) that in both cases the tight correlation is caused by a velocity-distance relation and hence that the redshifts are cosmological.

M. SCHMIDT

Hale Observatories,
California Institute of Technology,
Carnegie Institution of Washington,
Pasadena, California

Received January 19, 1973.

¹ Hoyle, F., *Nature*, **242**, 108 (1973).

² Schmidt, M., *Nature*, **240**, 399 (1972).

³ Sandage, A., *Astrophys. J.*, **178**, 25 (1972).

⁴ Sandage, A., *Astrophys. J.*, **178**, 1 (1972).

Multistatic Incoherent Scatter Measurements of Ionospheric Drift Velocity

THE observed behaviour of the ionospheric F-region has proved difficult to understand, and one of the principal difficulties has been that, until recently, there was little reliable information about the true plasma drift velocities. Apparent horizontal drift speeds have been measured for many years

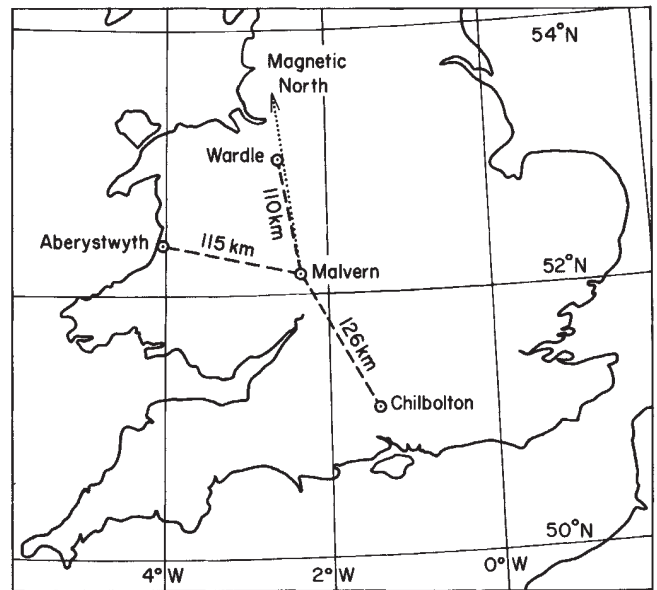


Fig. 1 Locations of the transmitter at Malvern and receivers at Chilbolton, Aberystwyth and Wardle. The angle between magnetic north and the Malvern-Wardle line, actually only 1° , is slightly exaggerated for clarity.

by means of the "fading" method¹: interpretation of data from these experiments is complex², and it is now clear that the results give only the drift of large electron density irregularities which, although of considerable interest in itself, is not necessarily the same as the drift of the plasma as a whole. Plasma velocity measurements have been made by plotting the movements of luminous chemical tracers released in the ionosphere from rockets or gun launched projectiles^{3,4}. Information from this type of experiment is of good quality, but is necessarily limited in quantity. An alternative technique is incoherent scatter radar, which measures the doppler shift of the scattered signal and determines one component of the ion drift velocity; the advantages of this method are that measurements can be pursued systematically as functions of time over a wide range of heights and that systematic errors can be minimized by careful equipment design. Incoherent scatter drift measurements were first made by Carru *et al.*⁵, who measured one component of the velocity, aligned nearly parallel to the direction of the geomagnetic field. A large body of similar measurements from the French installation now exists, and has been used to study thermospheric dynamics⁶. The incoherent scatter radar at Jicamarca, Peru, has measured the field perpendicular component near the magnetic equator⁷, and the vertical component has been measured by the Millstone Hill radar in Massachusetts⁸.

The field aligned component of ion drift is attributable to plasma diffusion and neutral air winds; above 150 km, the field perpendicular component is due only to electrostatic fields. It is therefore desirable to measure all components of drift velocity. Some work has been reported in which a steerable monostatic radar has been pointed obliquely in several directions successively, so as to measure more than one velocity component. Thus Evans⁹ has determined both horizontal components of the velocity vector and Harper¹⁰ has been able to measure all three orthogonal components. These measurements are necessarily made at different points in the ionosphere at different times, and while the mid-latitude ionosphere generally varies sufficiently slowly in space and time that the measurements can be treated as coincident, it would be more satisfactory to make measurements simultaneously in the same scattering volume. We present here multi-component incoherent scatter drift measurements which are coincident in space and time; these were achieved by receiving the scattered signals at more than one site.

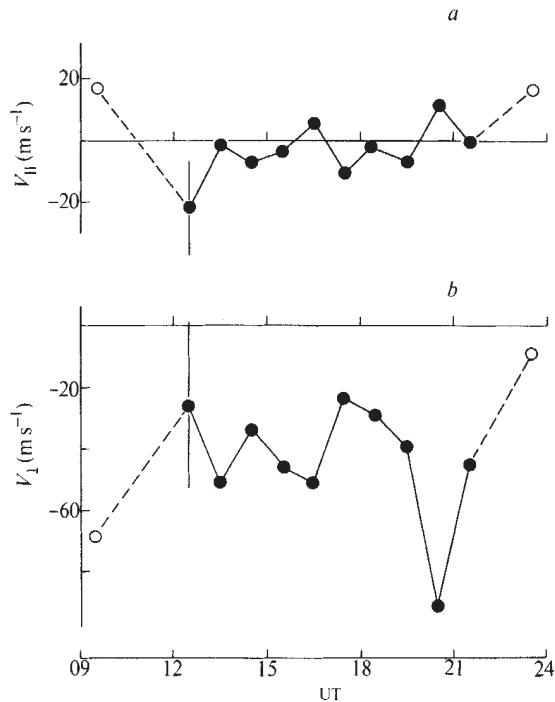


Fig. 2 Hourly averages of $V_{||}$ (positive upwards) and $V_{\perp}$ (positive northwards) at 270 km height above Malvern on April 17, 1972, calculated from Chilbolton and Wardle data. Open circles represent uncertain values. The vertical bars show typical errors, $\pm 10 \text{ m s}^{-1}$ for $V_{||}$ and $\pm 20 \text{ m s}^{-1}$ for $V_{\perp}$.

The equipment is situated at four sites and is an extension of the high power pulsed radar described previously¹¹. The transmitter is at Malvern (52.1° N, 2.3° W) and uses a 43 m diameter vertically pointing circular paraboloid to transmit a continuous wave of 40 kW mean power at a radio frequency of 400.50 MHz. The three receiving sites are at Chilbolton (51.1° N, 1.4° W), Jodrell Bank (Wardle, 53.1° N, 2.6° W) and Aberystwyth (Capel Dewi, 52.4° N, 4.0° W): their distances from Malvern are 126 km, 110 km and 115 km respectively at well separated azimuths (Fig. 1). The aerial at Chilbolton is a 25 m diameter circular paraboloid¹², and that at Wardle a 25 × 36 m elliptical paraboloid¹³, both fully steerable. The Aberystwyth aerial consists at present of two 12 × 25 m parabolic cylinders, connected to form a single aperture, movable in elevation. These three aeriels receive signals incoherently scattered from the plasma in a scattering volume vertically above Malvern. The height h of the scattering volume is selected by pointing the receiving aeriels at appropriate elevation angles, and the height resolution Δh depends on the beamwidth of these aeriels (about 2°), being about 13 km in the E layer at $h = 120 \text{ km}$ and 40 km in the F2 layer at $h = 275 \text{ km}$.

The signal characteristics measured are the signal-to-noise power ratio and the spectral distribution of signal power. At each receiving station, the 50 kHz intermediate frequency signal is clipped and recorded on magnetic tape as one-bit samples of the waveform. The tapes are later played back, using a 210 channel correlator which computes the autocorrelation function, integrating over a suitable period. A digital computer is used to transform the autocorrelation function into the corresponding power spectrum, and a best fitting theoretical spectrum determined¹⁴, yielding as parameters the electron and ion temperatures, T_e and T_i , and one component of drift velocity, V_m . We define V_m as positive when it is directed upwards. By the use of a pure c.w. transmitted signal, instrumental distortions of the spectra are minimized, and good fits between observed and calculated spectra can be obtained. Thus, unreliable spectra (for example, those contaminated by r.f. interference) can be eliminated readily by inspection. The errors of measurement by day, with good signal strength and about 20 min integration, are $\pm 15 \text{ K}$ for

T_e and T_i and about $\pm 12 \text{ m s}^{-1}$ for V_m . During the winter night the temperature error increases to about $\pm 100 \text{ K}$ and the velocity error to about $\pm 70 \text{ m s}^{-1}$; the velocity data are then not accurate enough to be useful. In contrast, the possible systematic error due to frequency instabilities in the system (less than 1 part in 10^8) is only about $\pm 1 \text{ m s}^{-1}$. The temperatures determined simultaneously at two or three receiving sites nearly always agree within their estimated errors, as expected.

To date, most of the observations have been made at Chilbolton and Wardle only. Measurements at two stations simultaneously do not suffice to determine the velocity vector completely, but only its projection on a certain plane. In the case of Chilbolton and Wardle, it is a reasonable approximation to assume this plane to be that of the magnetic meridian, which is inclined to the Chilbolton–Malvern line by only 20° and to the Malvern–Wardle line by only 1° (Fig. 1). If this approximation is made, the velocities V_m measured at the two stations can be combined to give the field-aligned component of drift, $V_{||}$, and the perpendicular component, $V_{\perp}$, in the meridian plane. The approximation fails if the drift velocity in the magnetic east–west direction is very much larger than that in the meridian plane, but it seems most unlikely that this will be the case generally. It so happens that the directions of the measured values of V_m make rather small angles with the magnetic field (1°–15° for Wardle and 30°–45° for Chilbolton, depending on height) and in consequence $V_{||}$ is more accurately determined than $V_{\perp}$.

Fig. 2 shows values of $V_{||}$ and $V_{\perp}$ (plotted as hourly averages) at 270 km height on a magnetically undisturbed day, April 17, 1972. $V_{||}$ varies slowly with time and is not greater than 20 m s^{-1} , the fluctuations probably being statistical. On this day $V_{\perp}$ exceeded $V_{||}$ in magnitude and was directed consistently

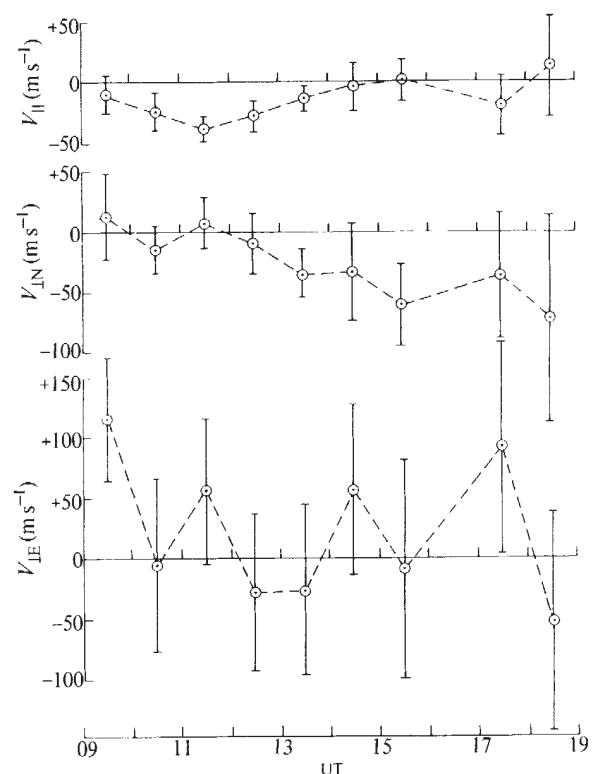


Fig. 3 Drift velocities at 270 km height deduced from simultaneous measurements at Chilbolton, Aberystwyth and Wardle. The component parallel to the geomagnetic field is $V_{||}$, reckoned positive upwards; the components perpendicular to the geomagnetic field are $V_{\perp N}$ (in the magnetic meridian, positive northwards) and $V_{\perp E}$ (in the magnetic eastward direction). Data obtained on November 15 and November 16, 1972, have been averaged.

southward from 1230 to 2130 UT at least, and probably for longer. On this day at least V_1 was opposite in direction to the theoretically calculated S_q drift, which is poleward during the day at middle latitudes¹⁵. But the diurnal variations of V_{11} and V_1 observed on other days are different, particularly when magnetic activity is present, and we cannot say yet whether the values in Fig. 2 are typical even of quiet days. Fig. 3 shows drift velocity components deduced from simultaneous measurements at all three receiving sites; further measurements of this type are being made, and will be presented later.

We thank Professor Sir Bernard Lovell for the use of the Jodrell Bank Mark III radio telescope at Wardle, Professor W. J. G. Beynon for his support, and G. W. King, P. H. McPherson and R. J. Risk for their help.

This work was carried out while G. N. T. was seconded to the Radio and Space Research Station.

G. N. TAYLOR

Royal Radar Establishment,
Malvern, Worcestershire

H. RISHBETH

Radio and Space Research Station,
Slough, Buckinghamshire

P. J. S. WILLIAMS

University College of Wales,
Aberystwyth, Cardiganshire

Received January 10, 1973.

- ¹ Mitra, S. N., *Proc. IEE*, **96-III**, 441 (1949).
- ² Jones, D., and Maude, A. D., *J. Atmos. Terrest. Phys.*, **34**, 1241 (1972).
- ³ Föpl, H., Haerendel, G., Haser, L., Lüst, R., Melzner, F., Meyer, B., Neuss, H., Rabben, H.-H., Rieger, E., Stöcker, J., and Stoffregen, W., *J. Geophys. Res.*, **73**, 21 (1968).
- ⁴ Murphy, C. H., Bull, G. V., and Edwards, H. D., *J. Geophys. Res.*, **71**, 4535 (1966).
- ⁵ Carru, H., Petit, M., and Waldteufel, P., *C.R. Acad. Sci. (Paris)*, **264**, 560 (1967).
- ⁶ Vasseur, G., *Ann. Geophys.*, **25**, 517 (1969).
- ⁷ Woodman, R. F., *J. Geophys. Res.*, **75**, 6249 (1970).
- ⁸ Evans, J. V., Julian, R. F., Reid, W. A., and Carpenter, L. A., *Radio Sci.*, **5**, 27 (1970).
- ⁹ Evans, J. V., *J. Geophys. Res.*, **77**, 2341 (1972).
- ¹⁰ Harper, R. M., *Space Res.* (in the press).
- ¹¹ Hey, J. S., Roberts, J. B. G., Taylor, G. N., and Watkins, C. D., *Nature*, **220**, 865 (1968).
- ¹² Meadows, R. W., *Electronics and Power*, **13**, 193 (1967).
- ¹³ Palmer, H. P., and Rowson, B., *Nature*, **217**, 21 (1968).
- ¹⁴ Powell, M. J. D., *Comput. J.*, **8**, 303 (1965).
- ¹⁵ Matsushita, S., *Space Res.*, **12**, 1087 (1972).

Location of the Proto-Atlantic Suture in the British Isles

WILSON¹ postulated, using faunal, tectonic and stratigraphic evidence, that the northern and southern parts of the British Isles were once separated by an ocean (the proto-Atlantic Ocean) which has since been destroyed by a continental drifting process in which the British Isles resulted from the welding together of land masses originally on different sides of the ocean. Following the stimulus of Wilson's idea and the possibility of plate tectonic theory providing an explanation for the continental movements, the geology of the central British Isles has been reappraised²⁻⁷ and the igneous, structural and stratigraphic features of the area have been related to the motion of lithospheric plates being thrust under continental masses as the continents converged.

Dewey and Pankhurst⁴ postulate that a plate thrust northwards under the Scottish Highlands is consistent with the orogenic features of the area. Fitton and Hughes⁷ have shown from petrochemical analyses that the regional variation in magma type in the Ordovician volcanic rocks of England is consistent with what would be produced by a plate being thrust

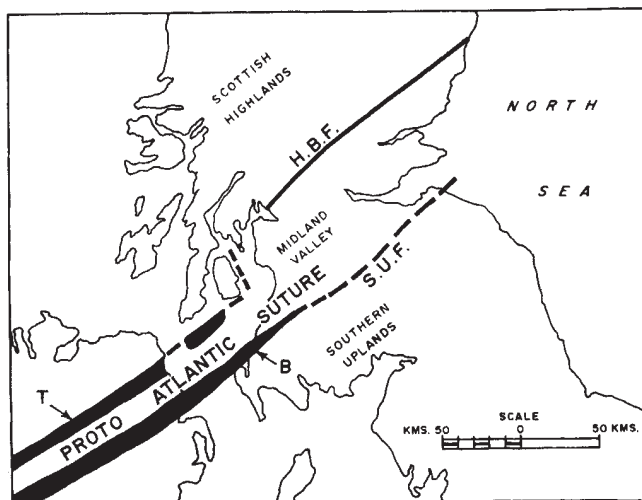


Fig. 1 Suggested location of the Proto-Atlantic Suture in Scotland and Northern Ireland. Black areas are ophiolitic rocks, known and deduced from geophysics. Dashed lines are faults, known and deduced from geophysics. H.B.F., Highland Boundary Fault; S.U.F., Southern Uplands Fault; B, Ballantrae Ophiolite Complex; T, Tyrone Igneous Complex. I believe there has been a tear in the oceanic plate along the line of Arran causing the offset of the line of ophiolites. I have interpreted a magnetic anomaly in the North Channel (shown as the isolated black area west of Arran) as an isolated wedge of ophiolites on the continuation of the Irish zone. There is no magnetic evidence to indicate any fault along the traditional line of the Highland Boundary Fault (just south of the Mull of Kintyre). The Tertiary igneous centre of Arran could well have been located by this tear in the lithospheric plate.

southwards. Although the diverging plate model appears reasonable there is an inconsistency because both models position their subduction zones along the line of the Southern Uplands fault where the ophiolitic rocks at Ballantrae are thought to be fragments of oceanic crust emplaced during the underthrusting.

In these models the Southern Uplands are considered to overlie oceanic crust compressed between two continental masses²⁻⁶; however, the geophysical evidence reviewed by Powell⁸ indicates that the Southern Uplands are underlain by a crust of normal continental thickness (30 km) and that thickening by compression from an oceanic crust (average thickness 7 km) is unreasonable. The estimates of Agger and Carpenter⁹ confirm the results described by Powell.

Another problem with considering the Southern Uplands as overlying oceanic crust is that andesitic volcanics (of Caradocian age) occur at Bail Hill near Sanquhar within the Southern Uplands¹⁰. Dewey⁸ admits that andesitic rocks would not be expected over an area of oceanic crust and postulates their present position to be the result of a moving seafloor. It is difficult to imagine this happening because according to Dewey's model the andesites occur between two plates being underthrust in different directions away from the andesites.

Compilations of gravity data in the area¹¹⁻¹⁴ show that there is no gravity high over the Southern Uplands as would be expected over a thin crust or a crust formed of dense basic material. The Midland Valley, however, does coincide¹³ with a large regional gravity high and this feature has induced me to consider the evidence that the Midland Valley may mark the proto-Atlantic suture.

The Midland Valley¹³ consists of a graben flanked by the Southern Uplands fault and the Highland Boundary fault and associated with each of these faults is a zone of ophiolitic rocks. The assemblage of black shales, cherts, spilites, gabbros and serpentinized peridotites of the Ballantrae Complex¹⁴ can be traced, using aeromagnetic data¹⁷, across into Northern Ireland (Fig. 1). A similar assemblage of rocks occurs in a thin zone along the Highland Boundary in Scotland¹⁸ and these rocks seem, on the basis of published gravity¹⁹ and aeromag-

netic¹⁷ data, to correlate with the ophiolitic Tyrone Igneous Complex¹² of Northern Ireland.

The absence of blue schists along the Highland Border region does not necessarily mean that this is not a subduction zone because the mode of emplacement of the ophiolites may have occurred without high pressure effects, for example they may have been detached from the lithospheric plate as a single slice of material.

It must be admitted that the position of this zone is not definite under the North Channel and there is the possibility of a north-south displacement in the vicinity of Arran. Because ophiolites are now believed to be fragments of oceanic crust emplaced by thrusting, welding and scraping along the zones where mobile oceanic crust underthrusts continental crust²⁰, the two zones of ophiolites flanking the Midland Valley are likely to mark two subduction zones. I thus consider the Midland Valley to be the oceanic remnant of the proto-Atlantic Ocean. This model means that the Highland Boundary fault and the Southern Uplands fault mark the positions of diverging Benioff zones and that the Midland Valley consists of a basement of oceanic origin overlain by sediments eroded from the flanking continental areas.

P. J. GUNN *

Geology Department,
University of Durham

Received November 8, 1972.

*Present address: Department of Economic Geology, University of Adelaide, South Australia.

¹ Wilson, J. T., *Nature*, **211**, 676 (1966).

² Dewey, J. F., *Nature*, **222**, 124 (1969).

³ Dewey, J. F., *Scott. J. Geol.*, **7**, 219 (1971).

⁴ Dewey, J. F., and Pankhurst, R. J., *Trans. Roy. Soc. Edinb.*, **68**, 361 (1970).

⁵ Ziegler, A. M., *J. Geol.*, **78**, 445 (1970).

⁶ Bird, J. M., Dewey, J. F., and Kidd, W. S. F., *Nature Physical Science*, **231**, 28 (1971).

⁷ Fitton, J. G., and Hughes, D. J., *Earth Planet. Sci. Lett.*, **8**, 223 (1970).

⁸ Powell, D. W., *Scott. J. Geol.*, **7**, 369 (1971).

⁹ Agger, H. E., and Carpenter, E. W., *Geophys. J. Roy. Astron. Soc.*, **9**, 69 (1964).

¹⁰ Pringle, J., *The South of Scotland* (HMSO, 1948).

¹¹ De Bruyn, J. W., *Geophys. Prospect.*, **3**, 1 (1955).

¹² Charlesworth, J. K., *Historical Geology of Ireland* (Oliver and Boyd, London, 1963).

¹³ McLean, A. C., and Qureshi, I. R., *Trans. Roy. Soc. Edinb.*, **66**, 267 (1966).

¹⁴ Powell, D. W., *Scott. J. Geol.*, **6**, 355 (1970).

¹⁵ Macgregor, M., and Macgregor, A. G., *The Midland Valley of Scotland* (HMSO, 1948).

¹⁶ Greig, D. C., *The South of Scotland* (HMSO, 1971).

¹⁷ *Aeromagnetic Maps for Great Britain and Northern Ireland*, published by the Geological Survey of Great Britain.

¹⁸ Anderson, J. G. C., *Trans. Roy. Soc. Edinb.*, **61**, 419 (1947).

¹⁹ *Gravity Map of Northern Ireland*, published by the Geological Survey of Northern Ireland.

²⁰ Dewey, J. F., and Bird, J. M., *J. Geophys. Res.*, **76**, 3179 (1970).

Couple on a Bar Magnet

Whitworth and Stopes-Roe¹ have described experiments in which the couple on a bar magnet was measured with and without the magnet being immersed in a paramagnetic fluid, and have interpreted their results as supporting the Kennelly formulation in which the couple on a magnet is proportional to H rather than the Sommerfeld formulation which makes it proportional to B . But they add that they expect the couple on a magnet immersed in a medium to depend on its shape. Their experiments refer to long thin magnets, which they regard as true magnetic dipoles, and the Sommerfeld equations would be correct for magnetic shells or current loops.

If we accept these statements and wish to generalize the equations to cover magnets of any shape, the torque equations

must include a shape factor. Thus in the Kennelly system, the couple T on a magnet of moment j could be written

$$T = S_k j \times H \quad (1)$$

In the Sommerfeld system with moment m

$$T = S_s m \times B \quad (2)$$

To agree with Whitworth and Stopes-Roe the Kennelly shape factor S_k must be 1 for a long thin magnet and μ_r for a magnetic shell, and the Sommerfeld shape factor S_s must be 1 for a shell and $1/\mu_r$ for a long thin magnet; μ_r is the relative permeability of the medium.

I accept Whitworth and Stopes-Roe's experimental results and think their suggestion of a shape effect is probably correct. I disagree with their preference for long thin magnets, and would choose spheres, cubes or cylinders with equal lengths and diameters. Such shapes would not pre-empt the results in favour of either the Kennelly or Sommerfeld system. They also approximate best to ideal dipoles in the sense that the external field they produce conforms closely to the inverse cube law for dipoles.

The shape factor has been calculated by Smythe² for a sphere in a permeable medium. His derivation is preceded by the statement that a permanent magnet has the same permeability μ , as free space, and inserting a body of this permeability in a medium of permeability μ distorts the field at its boundaries. This distortion is due to inserting a vacuum into the medium and is quite independent of the actual magnetization of the magnet.

The equation derived by Smythe for the torque is

$$T = \frac{4\pi\mu_v a^3 MB \sin \alpha}{2\mu + \mu_v} \quad (3)$$

where a =radius and M =magnetization of the sphere in $A\ m^{-1}$.

If I introduce the relative permeability of the medium μ_r , equate $4\pi a^3 M/3$ to the Sommerfeld moment and use vector notation, equation (2) becomes

$$T = m \times B \frac{3}{2\mu_r + 1} \quad (4)$$

Substituting $m = j/\mu_v$ and $B = \mu_r \mu_v H$ gives the equivalent Kennelly form

$$T = j \times H \frac{3\mu_r}{2\mu_r + 1} \quad (5)$$

Thus the shape factor for the sphere is $3/(2\mu_r + 1)$ in the Sommerfeld system and $3\mu_r/(2\mu_r + 1)$ for the Kennelly system. If shape factors are ignored, an experiment similar to that of Whitworth and Stopes-Roe carried out on magnets that are neither long bars nor thin disks would seem to disprove both systems.

Some people prefer to avoid these shape factors by saying that immersing the magnet in a medium changes its moment. They are quite entitled to this point of view. It implies that equation (1) or (2) without shape factors does not predict the torque but defines a new concept, "the magnetic moment of a magnet in a medium". No further use can be made of this moment without calculating the shape factor that relates it to the moment in free space. The present standpoint that the magnetic moment of a uniformly and permanently magnetized body is unchanged by surrounding it by a magnetic medium is equally valid and in my opinion somewhat more convenient. Of course, a non-uniformly magnetized body in a non-uniform field presents additional problems and it is probably not meaningful to speak of its moment.

With the correct use of shape factors either the Kennelly or Sommerfeld system can be used. If, however, shape factors

are necessary to predict the torque on a permanent magnet, should not the question of forces on a current-carrying conductor immersed in a magnetic medium be re-examined? Consider as an example a thin film-shaped conductor immersed in a medium of relative permeability μ_r . The permeability of the conductor is identical with that of a vacuum just as Smythe assumed for a magnet and both $\mathbf{B}$ and $\mathbf{H}$ are parallel to the plane of the film. It is certain that $\mathbf{H}$ in the film is precisely the same as $\mathbf{H}$ in the medium and, also, for an experiment similar to that of Whitworth and Stopes-Roe, neglecting the self-demagnetizing field of the tank, it is the same as if the paramagnetic liquid were removed. $\mathbf{B}$ in the conductor is also the same as if the liquid were removed. If the electric current is due to electrons moving in a medium with the same permeability as free space, it is difficult to avoid the conclusion that the force on these electrons is not affected by surrounding the conductor with a magnetic medium.

If, however, the plane of the conductor is turned so that it is normal to the field, both $\mathbf{B}$ and $\mathbf{H}$ within the conductor are increased. To assume, however, that the force depends on the orientation of the conductor leads to difficulties—two current-carrying conductors should experience equal and opposite forces. There seem to be two possible solutions to this paradox. One is to suppose that the force on a conductor whatever its orientation is unaffected by a surrounding medium. This could be justified by saying that any change in $\mathbf{B}$ or $\mathbf{H}$ when the medium is introduced arises from unanchored fluid molecules incapable of exerting a force on the conductor. An alternative suggestion was made during a discussion on this subject at the June 1972 meeting of the Magnetism Group of the Institute of Physics in Oxford. According to this suggestion part of the force on the conductor may arise from a hydrostatic pressure gradient in the fluid. Such a force might vindicate the generally accepted assumption that the force on a current-carrying conductor is proportional to $\mathbf{B}$.

Whitworth and Stopes-Roe have shown that these questions can be investigated experimentally, and more experiments are clearly desirable. In general in such experiments, the self-demagnetizing field of the tank may need to be considered, although Whitworth and Stopes-Roe seem to have eliminated this factor with their particular arrangement.

I thank Dr F. J. Lowes for comments and for drawing my attention to ref. 2.

M. McCRAIG

Permanent Magnet Association,
Central Research Laboratory,
84 Brown Street,
Sheffield S1 1FA

Received December 7, 1972.

¹ Whitworth, R. W., and Stopes-Roe, H. V., *Nature*, **234**, 31 (1971).

² Smythe, W. R., *Static and Dynamic Electricity*, 357 (McGraw-Hill, New York, 1960).

Preparation of Tritium-labelled Talc

TALC, a native hydrous magnesium silicate approximating to the general formula $\text{Mg}_3\text{Si}_4\text{O}_{10}(\text{OH})_2$, is extensively used in industry in addition to its wide acceptance as a cosmetic and toiletries product. Although chemically inert, it produces a number of biological effects including tissue granulomata^{1,2} and a form of pneumoconiosis after inhalation^{3,4}. A recent report⁵ suggesting an association of cancer of the cervix and ovaries with the presence of talc particles at these sites has caused disquiet about its safety in use. Although the evidence is equivocal, it follows the acceptance of a causal relationship between exposure to asbestos dust and mesothelioma⁶.

The detection of talc particles in tissues, essential for the elucidation of the mechanisms of pathogenicity, presents considerable difficulties. Although the extraction replication technique developed by Henderson *et al.*⁷ offers an elegant

means for the identification of talc particles in tissues, the small areas that can be surveyed by electron microscopy do not provide an overall picture of particle distribution. Furthermore, the adventitious contamination of tissues during the preparation of specimens for examination cannot be ruled out. The value of using a suitably labelled radioactive talc for such studies is self-evident, and the use of tritium-labelled asbestos⁸ has greatly facilitated the localization of asbestos fibres in tissues.

A method has now been developed for the preparation of tritium-labelled talc. The process involves two stages; first, the partial dehydration of talc under controlled conditions, and second, the rehydration of the material using tritiated water. The talc used conformed with the specifications of the British Pharmacopoeia 1968.

The partial dehydration of talc was effected by heating talc in a platinum dish at 835–840° C for 1 h resulting in the loss of 1 molecule of H_2O . The study of the dehydration kinetics of talc showed that below this temperature no loss of H_2O occurred, and that at temperatures in excess of 900° C the second molecule of H_2O was lost, gradually leading eventually to complete dehydration at 1,000° C. The reincorporation of 1 molecule of H_2O into the partially dehydrated talc was achieved by refluxing the material with 50 volumes of benzene/water azeotropic mixture (92 : 8) for 2 h. The talc was filtered on a No. 4 sintered glass filter crucible and the last traces of solvents removed at 130° C under vacuum. Electron microscopy and X-ray diffraction studies showed that the rehydrated material was virtually identical with and indistinguishable from the original talc. It was found that talc could be repeatedly subjected to the processes of partial dehydration and rehydration, employing the methods described, without any substantial alteration in its physical properties or chemical composition.

The efficiency of the rehydration procedure was considerably improved by heating the partially dehydrated talc and 5 volumes of benzene/water azeotropic mixture contained in sealed ampoules in an autoclave at 15 pound inch⁻² pressure for 30 min. The ampoules were opened, centrifuged at 3,000 r.p.m. for 5 min and the supernatant liquid decanted. The rehydrated talc was washed twice with distilled water, recentrifuged and finally dried at 130° C under vacuum. This procedure was adopted for the incorporation of tritiated water into talc, and the inclusion of tritiated water into the benzene/water mixture (0.5 mCi ml⁻¹) resulted in the formation of tritium-labelled talc (15,000 d.p.m. mg⁻¹). The thermal stability of this labelled material was similar to that of the rehydrated talc and there was no evidence of exchange with H_2O at 100° C.

Biological investigations on the tissue distribution of the tritium-labelled talc are currently in progress.

We thank Mr J. K. Foreman for X-ray diffraction studies.

S. D. GANGOLLI
R. F. CRAMPTON
A. G. LLOYD

The British Industrial Biological Research Association,
Woodmansterne Road,
Carshalton, Surrey

Received January 17, 1973.

¹ Tye, M. J., Hashimoto, K., and Fox, F., *J. Amer. Med. Ass.*, **198**, 1370 (1966).

² Migaki, G., and Garner, F. M., *Amer. Vet. Med. Ass.*, **155**, 1595 (1969).

³ Van Ordstrand, H. S., *Chest*, **58**, 2 (1970).

⁴ Miller, A., Teirstein, A. S., Bader, M. E., Bader, R. A., and Selikoff, I. J., *Amer. J. Med.*, **50**, 395 (1971).

⁵ Henderson, W. J., Joslin, C. A. F., Turnbull, A. C., and Griffiths, K., *J. Obs. Gyn. Br. Comm.*, **78**, 266 (1971).

⁶ Selikoff, I. J., Bader, R. A., Bader, M. E., Churg, J., and Hammond, E. G., *Amer. J. Med.*, **42**, 487 (1967).

⁷ Henderson, W. J., Harse, J., and Griffiths, K., *Europ. J. Cancer*, **5**, 621 (1969).

⁸ Turncock, A. C., Bryks, S., and Bertalanffy, F. D., *Environ. Res.*, **4**, 86 (1971).

BIOLOGICAL SCIENCES

A Unique RNA Species from Maturing Mouse Spermatozoa

STUDIES on ejaculated spermatozoa have shown that RNA is either absent or present only in trace amounts¹⁻⁴. Work on RNA synthesis during spermatogenesis, however, suggested that RNA might be present in maturing spermatozoa. Although histochemical techniques disclosed little cytoplasmic RNA after the second meiotic division⁵, Monesi's autoradiographic studies on mouse testes have demonstrated a small burst of post-meiotic nuclear RNA synthesis that declined upon spermatozoan maturation⁶. Moore has also shown active RNA polymerase in early spermatids⁷. Although RNA has not been quantitatively detected in ejaculated spermatozoa, incorporation of radioactive RNA precursors has been demonstrated⁸ and we have extended these findings. Our studies on RNA metabolism in maturing mouse spermatozoa (from the epididymis and the ductus deferens) reveal the presence of larger amounts of RNA than previously reported for ejaculated spermatozoa. We have not investigated the RNA content of the ejaculated spermatozoa.

The epididymis and ductus deferens were dissected from mature random bred Swiss mice and placed in a small Petri dish. Spermatozoa were stripped from the ductus deferens, and the epididymides were chopped with a sharp razor blade. The chopped tissue was suspended in 4 ml./mouse of Beatty's media⁹, swirled for 5 min and then strained through a wire screen (16 mesh) into a centrifuge tube. The sample was centrifuged at 10*g* for 30 s to pellet contaminating cells. The spermatozoa were transferred to a clean centrifuge tube, counted in a haemocytometer and centrifuged for 15 min at 600*g*. No contaminating cells (except for about 1%, RNA-free erythrocytes) were seen in the haemocytometer or in fixed smears.

The content of RNA in the sperm pellet was determined by the orcinol method¹⁰. RNA was extracted at 55° C with phenol and chloroform^{11,12}, treated with iodoacetate treated¹³ beef pancreatic DNAase I (Worthington, electrophoretically pure), and extracted further with cold phenol and chloroform. The RNA sample was dissolved in an appropriate amount of 0.04 M Tris, 0.02 M sodium acetate, 0.001 M EDTA, 0.5% SDS at pH 7.8.

Table 1 RNA Content of Mouse Spermatozoa

Spermatozoa sample	pg RNA sperm (ribose determinations on whole sperm)	% Hydrolysed with RNAase	pg RNA sperm RNA extraction yields (OD 260)
Total sperm*	0.476±0.071	67.2±19	0.167±0.026
Caput epididymal sperm	0.648±0.083	—	0.260±0.012
Cauda epididymal sperm	0.236±0.030	—	0.091±0.007
Ductus deferens sperm	0.592±0.112	—	0.258±0.112

*Total sperm is a mixture of epididymal and ductus deferens spermatozoa and represents 26% from the ductus deferens, 50% from the cauda epididymis, and 24% from the caput epididymis (average haemocytometer counts).

Table 1 lists the RNA content of spermatozoa in the epididymides plus the ductus deferens, caput epididymis, cauda epididymis and ductus deferens. Total spermatozoa (from epididymis and ductus deferens) showed an average decrease of 67% upon pancreatic RNAase treatment (for 20 h at 37° C at pH 7.8) as determined by the ribose colorimetric assay¹⁰.

The amount of purified RNA was determined from OD 260 measurements by using a value of 214 for $E_{1\%}^{1\text{cm}}$ at 260 nm in 0.1 M NaCl¹⁴. Colorimetric assay on whole spermatozoa and RNA extraction yields (OD 260) show a similar trend for differences among the spermatozoa from the caput and cauda epididymis and ductus deferens. The drop in RNA content in the older cauda epididymis spermatozoa with a later increase in ductus deferens spermatozoa appears to indicate that RNA synthesis is accelerated or re-initiated in the ductus deferens spermatozoa. Variable contamination has been excluded.

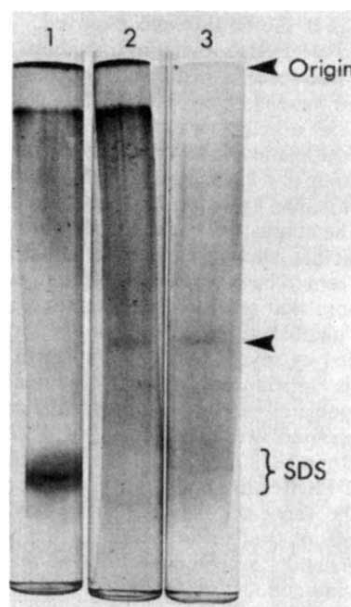


Fig. 1 Gel electrophoresis of purified epididymal and ductus deferens spermatozoa. RNA extraction was performed as described in the text, except at 40° C and with no DNAase treatment. The purified RNA was electrophoresed on polyacrylamide gels containing 2.4% acrylamide and 0.12% bisacrylamide. Gels were run at room temperature in a 0.04 M Tris, 0.02 M sodium acetate, 0.001 M EDTA buffer at pH 7.8 for 2 h at 5 mamp/gel. Gels were fixed for 15 min in 1 M acetic acid and stained for 1 h in 0.2% methylene blue. They were destained in distilled water. (1) Treated with pancreatic RNAase. (2) No enzymatic treatment. (3) Treated with pancreatic DNAase I.

Gel electrophoresis of the extracted RNA (Fig. 1) demonstrated the presence of an RNA band corresponding to a molecular weight of $190,000 \pm 5,000$ daltons (about 11S). This molecular weight represents 16 different gels from 10 different sperm samples and was obtained by measuring the electrophoretic migration distance and from a semi-log plot of molecular weight against migration distance. DNAase treatment has no effect on the 11S band, whereas RNAase treatment removes the band. The band that was removed with DNAase treatment was seen routinely in non-DNAase treated RNA samples purified from other tissues (liver and fibroblasts); it is purple (as opposed to blue for RNA) when stained with methylene blue, and appears to represent DNA fragments. Gel analysis of sperm RNA from the caput and cauda epididymis and ductus deferens gave identical results. A small molecular weight RNA (3S to 4S) has been seen with gel electrophoresis and SDS sucrose gradients when the sperm RNA was extracted at 55° C. This 4S band was not detected when the RNA was extracted at 40° C.

The presence of this 190,000 dalton RNA in mouse spermatozoa might be explained by one of the following: (1) The RNA seen could *a priori* be the result of contaminating cellular material. This possibility is highly unlikely because the method of preparing spermatozoa resulted in no microscopically detectable contaminating cells and ribosomal RNA is not seen upon gel electrophoresis or sucrose gradient centrifugation.

(2) It may be a histone mRNA, biosynthesis of an arginine rich histone (protamine) has been observed during late spermatogenesis^{6,13-17}. There is an increase in arginine and cystine and a decrease in other amino-acids as spermatozoa pass through the epididymis of the bull¹⁶. The 11S sperm RNA might be the message for this arginine rich histone. The electrophoretic migration (Fig. 1) corresponds to the migration seen for histone mRNA^{18,19}. As there is no ribosomal RNA present, we must assume that active protein synthesis is not occurring during this maturational stage of spermatogenesis. If the 11S sperm RNA is a message it may be left over from earlier protein synthesis, and be resistant to whatever mechanism resulted in the degradation of the ribosomal RNA. (3) The mitochondria of the spermatozoa may be the source of the 11S RNA. Mitochondrial 11S RNA has been seen in sea urchin embryos²⁰. Bands corresponding to the mitochondrial ribosomal RNA, however, were not seen for sperm RNA. (4) The 11S RNA may be the result of the RNA degradation which occurs naturally in the spermatozoa or as a result of the RNA extraction. Sheid has reported an unusual component in bull sperm that degrades RNA into non-dialysable fragments²¹. If the RNA is being degraded during the extraction, however, we would not expect to see the same sharp band from different samples and the migration patterns for all sperm RNA samples were identical. Also when sperm was extracted together with liver the electrophoretic pattern of the liver RNA was normal. (5) It may be a species of RNA or an RNA complex that is unique to sperm.

At present we are attempting to determine if sperm RNA is pre- or post-meiotic. Post-meiotic synthesis of RNA would indicate haploid gene expression, which must be postulated to explain *t*-allele segregation distortion of mice²² and which could explain deficient fertilization of human diplo-Y spermatozoa²³.

This work was supported by a grant from the National Institutes of Health.

C. J. BETLACH
R. P. ERICKSON

Department of Pediatrics,
University of California,
San Francisco, California 94122

Received November 13, 1972.

- ¹ Vendrely, R., and Vendrely, C., *Experientia*, **4**, 434 (1949).
- ² Mann, T., *Biochem. Soc. Symp.*, **7**, 11 (1951).
- ³ White, J. C., Leslie, I., and Davidson, J. W., *J. Path. Bact.*, **66**, 291 (1953).
- ⁴ Bhargava, P. M., Bishop, M. W. H., and Work, T. S., *Biochem. J.*, **73**, 242 (1959).
- ⁵ Daoust, R., and Clermont, Y., *Amer. J. Anat.*, **96**, 255 (1955).
- ⁶ Monei, Valerio, *J. Reprod. Fert., Suppl.*, **13**, 1 (1971).
- ⁷ Moore, G. P. M., *Exptl. Cell Res.*, **68**, 462 (1971).
- ⁸ Abraham, K. A., and Bhargava, P. M., *Biochem. J.*, **86**, 298 (1963).
- ⁹ Beatty, R. A., *Proc. 5th Int. Congr. Anim. Reprod. Art. Insem., Trento*, **3**, 276 (1964).
- ¹⁰ Habel, Karl, and Salzman, Norman, *Fundamental Techniques in Virology* (Academic Press, New York, 1969).
- ¹¹ Cooper, Herbert L., and Kay, John E., *Biochim. Biophys. Acta*, **174**, 503 (1969).
- ¹² Penman, S., *J. Mol. Biol.*, **17**, 117 (1966).
- ¹³ Cantoni, Giulio L., and Davies, David R. (edit.), *Proceedings of Nucleic Acid Research*, **1**, 98 (Harper and Row, New York, 1966).
- ¹⁴ Hall, B. D., and Doty, P., *J. Mol. Biol.*, **1**, 111 (1959).
- ¹⁵ Ingles, C. J., Trevithick, J. R., Smith, M., and Dixon, G. H., *Biochem. Biophys. Res. Commun.*, **22**, 627 (1966).
- ¹⁶ Lavon, U., Volcani, R., and Danon, D., *J. Reprod. Fert.*, **24**, 219 (1971).
- ¹⁷ Lam, D. M. K., and Bruce, W. R., *J. Cell Physiol.*, **78**, 13 (1971).
- ¹⁸ Adesnik, Milton, and Darnell, James E., *J. Mol. Biol.*, **67**, 397 (1972).
- ¹⁹ Schochetman, G., and Perry, R. P., *J. Mol. Biol.*, **63**, 591 (1972).
- ²⁰ Chamberlain, John P., and Metz, Charles B., *J. Mol. Biol.*, **64**, 593 (1972).
- ²¹ Sheid, Bertrum, and Wilson, Susan M., *Nature*, **232**, 628 (1971).
- ²² Gluecksohn-Waelsch, Salome, and Erickson, Robert P., *Curr. Topics Dev. Biol.*, **5**, 281 (1970).
- ²³ Sumner, A. T., Robinson, J. A., and Evans, H. J., *Nature*, **229**, 231 (1971).

Fusion of Rat and Mouse Morulae and Formation of Chimaeric Blastocysts

THE induction of chimaerism by the aggregation of blastomeres of different mouse strains is one of the most remarkable recent contributions of experimental embryology^{1,2}.

Last year it was pointed out (Dr A. McLaren, private communication) that the possibility to fuse morulae of different species had not been sufficiently explored³. I therefore undertook a series of experiments to investigate whether aggregation of rat and mouse morulae can be induced, using basically the same techniques as those for aggregation of mouse morulae^{3,4}.

Mouse morulae (C3Hf × Swiss, 8–16 cell stage) were flushed from the utero-tubal region with culture medium on day 2 of pregnancy at 2100 h, day 0 of plug, 14 h of light, 10 h of dark-

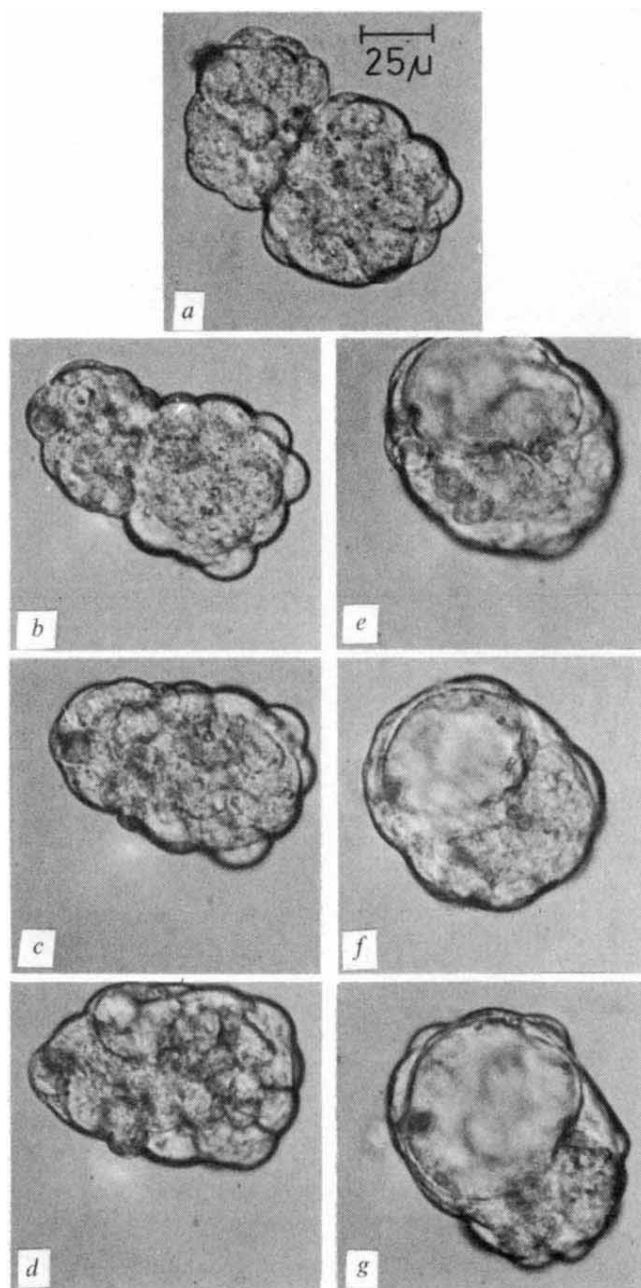


Fig. 1 a, Mouse (left) and rat morulae in apposition, shortly after withdrawal of egg holders. Developmental stages after 2 (b), 4 (c), 6 (d), 8 (e), 11 (f) and 12.5 h (g). In d the blastocoelic cavity is formed.

ness, the middle of the dark period at midnight. Rat morulae (R, Amsterdam Wistar, 8–16 cell stage) were isolated by flushing on day 3 at 2130 h. The isolated morulae were stored separately at 37° C under oil and 5% CO₂ in air in pyruvate containing culture medium⁵.

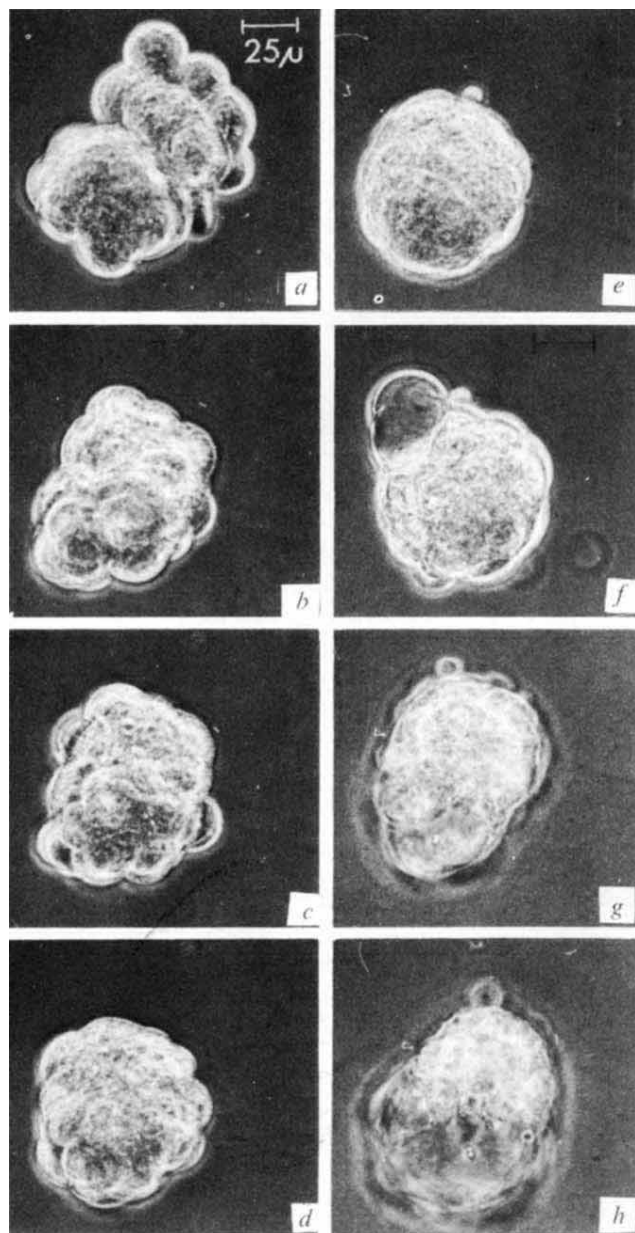


Fig. 2 Mouse (left) and rat (upper right) morulae in the process of fusion. Pictures taken after 30 min (a), 2 (b), 6 (c), 8 (d), 12.5 (e), 18.3 (f), 24 (g) and 34 h (h). Borderline between rat and mouse morulae still visible in (e). First cavity formed in (f), final cavity in (g).

The zona pellucida was dissolved by incubation of the eggs for 3 to 5 min in 0.5% pronase⁶. Final removal of the zona occurred in culture medium by forcing the eggs through a narrow pipette. In each experiment one mouse and one rat morulae were brought into contact in an oil drop culture at 37° C by means of 2 egg holders with a closed lumen⁷ driven by a micromanipulator. During this apposition phase, which lasted 20 to 30 min, the Petri dish was not closed but a humid gas stream was directed over the oil surface in order to maintain pH and osmolarity of the medium.

After removal of the egg holders the development of the pair of morulae was followed continuously with a time-lapse movie camera connected to the inverted microscope. Photographs were taken every 30 s with flash light.

A total of sixteen identical experiments were carried out and, in twelve, successful aggregation had occurred in which both aggregate partners showed active proliferation as judged by the temporary bulging of dividing cells at the surface of the large morulae. Sometimes the individual morulae could be recognized for a long time after aggregation by a slight indentation. The way in which the blastocoelic cavity formed varied. In certain cases it appeared where the junction between the two morulae could be seen earlier (Fig. 1b–d), in others an abortive blastocoelic cavity formed in one of the partners (Fig. 2f), which disappeared later. In the same aggregate the lasting cavity formed in the other partner (Fig. 2g). In one experiment one of the aggregate partners formed a cavity before the cell mass had become round (Fig. 3b); this disappeared and a final one was formed after aggregation (Fig. 3d). The pictures and analysis of the time-lapse photographs show that both rat and mouse cells contribute to the formation of a large blastocyst.

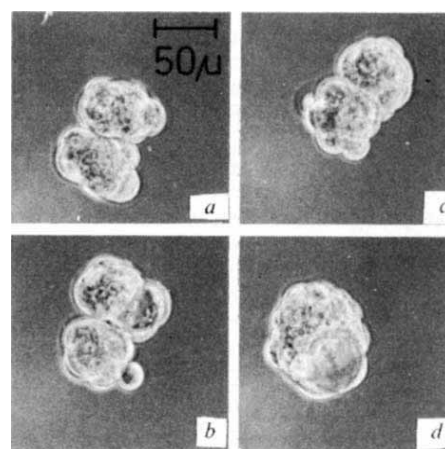


Fig. 3 Fusion of rat and mouse morulae after formation and disappearance of a cavity in the mouse morula. Pictures taken at approximately 30 min (a), 6.6 (b), 10 (c) and 19 h (d).

A general feature of the chimaeric blastocysts was that the inner cell mass was comparatively large.

Preliminary observations have shown that the developmental stage of the rat eggs is critical for successful aggregation and the use of a micromanipulator was also very helpful.

A copy of the time-lapse movie is available for study.

G. H. ZEILMAKER

*Department of Endocrinology, Growth and Reproduction,
Erasmus University,
PO Box 1738, Rotterdam*

Received September 29; revised October 20, 1972.

- ¹ Tarkowski, A., *Nature*, **190**, 857 (1961).
- ² Mintz, B., *J. Exp. Zool.*, **157**, 273 (1964).
- ³ Mintz, B., in *Preimplantation Stages of Pregnancy*, CIBA Foundation Symposium (edit. by Wolstenholme, G. E. W., and O'Connor, M.), 207 (Churchill, London, 1965).
- ⁴ Tarkowski, A., in *Preimplantation Stages of Pregnancy*, CIBA Foundation Symposium (edit. by Wolstenholme, G. E. W., and O'Connor, M.), 183 (Churchill, London, 1965).
- ⁵ Biggers, J., Wittingham, D. G., and Donahue, R. P., *Proc. US Nat. Acad. Sci.*, **58**, 560 (1967).
- ⁶ Mintz, B., *Science*, **138**, 594 (1962).
- ⁷ Lin, T. P., *Science*, **151**, 333 (1966).

Effects of Cannabis and Alcohol on Psychological Tests

WE report here psychological data from a combined study on the effect of cannabis and alcohol on simulated car-driving and on psychological tests. The car simulator data showed an effect of cannabis and alcohol on brake time and start time, and a much more pronounced effect of cannabis than of alcohol on time and distance estimation after simulated driving. We have also measured the dose-response curve for cannabis¹. This report deals with tests of attention and short-term memory, and of subjective and objective ratings of degree and kind of intoxication.

Many of the details are described in ref. 1 and 16. Placebo and double-blind procedures, dose response measurements, and tests for reproducibility and training effects were used, and the subjects acted as their own controls. The effect of a standard dose of alcohol was also studied.

The eight volunteers were healthy men, 21–29 yr old. Three were college graduates and five were skilled workers. None were alcohol or cannabis abusers (that is, no one had a daily alcohol consumption exceeding four drinks; three had never tried cannabis, five had used cannabis from one to fifteen times, nobody more than once a week). The psychological test battery consisted of (A) cognitive tests, (B) consciousness and mood scales, and (C) personality tests. The cognitive tests included: (1) digit span; (2) addition test; (3) subtraction test (serial subtraction of sevens); (4) finger labyrinths (constructed of cardboard plates and thin wooden sticks; the subjects were blindfolded and four different degrees of complexity were used); and (5) Bourdon's cancellation test.

The consciousness and mood scales included (6) Smith and Beecher's mood questionnaire² (self-rating scale: the twelve items were rated on a seven-point scale; in our design two more items were added: "motivation" and "kind and degree of intoxication"); (7) an objective questionnaire (rating by experimenter of eleven items covering similar modalities as the Smith and Beecher's questionnaire). The personality tests were for obvious reasons not part of the daily programme and will be described in a later communication, but included (8) the Minnesota Multiphasic Personality Inventory (MMPI) (this test was used during the primary selection of subjects; only subjects without pathological traits according to MMPI profiles were admitted to the study); (9) the Rorschach test; (10) association tests; and (11) interviews.

Both cannabis and alcohol were administered by a double-blind technique and in random order. As a pre-test the subjects were first submitted to the psychological test battery, and driving in the car-simulator. The drug tests were administered 105 min after intake of the cannabis cake (without or with Δ^1 -THC), and 75 min after a drink of 500 ml. fruit juice (without or with 70 g alcohol), respectively. They consisted of a parallel battery of psychological tests, estimation of kind and degree of intoxication by subject and experimenter, separately, and repetition of simulated car-driving. The next morning (16 h from zero time) the procedure was repeated: (1) a parallel battery of psychological tests; (2) repetition of simulated car-driving; and (3) interview on drug (or placebo) experience. The main research period covered 9 weeks. Each subject completed 9 research days with weekly intervals. The 9-week period was subdivided into three 3-week periods or parts. Part I and part II were designed to compare cannabis and alcohol effects, part II being an exact repetition of part I, except for a new rotation due to continued randomization. Part III was designed to study the dose-response relation for cannabis.

Cannabis resin in original form and baked into cakes was analysed in three different laboratories in Denmark and the USA by gas-chromatography and thin-layer chromatography. Consistent results indicated that the Δ^1 -THC content was 4%; 200, 300, and 400 mg cannabis resin thus correspond-

ing to 8, 12, and 16 mg Δ^1 -THC. The inactive cannabis resin used for placebo cakes had a negligible THC content¹.

Non-parametric statistics³ (Wilcoxon's matched pairs signed ranks test) were used throughout the study. To counter-balance the effects of practice, day-to-day and test-to-test variation, each subject was his own control, both on each research day and in between-days calculations. The results from part I and part II were pooled in the final analysis, because training effects were found to be small compared to drug induced changes.

Table 1 Average Time Spent on Addition by Eight Subjects

Treatment	Pre-test (s)	Drug test (change %)	Post-test (change %)
Placebo	22.7	6	3
Cannabis 300 mg	22.4	37*	8
Alcohol 70 g	22.5	28	1
Cannabis 200 mg	22.1	7	3
Cannabis 300 mg	21.1	37	0
Cannabis 400 mg	20.3	56*	12

Average time spent on addition of four vertical rows of twenty single digits. Change in per cent indicates differences between drug test or post-test and pre-test values. Top half is accumulated data from parts I and II (cannabis versus alcohol), bottom half from part III (dose-response relationship for cannabis).

* $P < 0.05$.

Table 2 Average Time Spent on Subtraction by Eight Subjects

Treatment	Pre-test (s)	Drug test (change %)	Post-test (change %)
Placebo	25.8	0	-2
Cannabis 300 mg	25.6	57*	0
Alcohol 70 g	26.5	46	-2
Cannabis 200 mg	21.3	9	9
Cannabis 300 mg	21.4	21*	3
Cannabis 400 mg	23.3	57*	-10

Average time spent on serial subtraction of sevens starting from a number between 100 and 106. Change (%) indicates differences between drug test or post-test and pre-test values. Top half is accumulated data from parts I and II (cannabis versus alcohol), bottom half from part III (dose-response relationship for cannabis).

* $P < 0.05$.

Table 3 Average Time Spent on the Finger Labyrinth by Eight Subjects

Treatment	Pre-test (s)	Drug test (change %)	Post-test (change %)
Placebo	94.9	4	-1
Cannabis 300 mg	99.4	19*	-12
Alcohol 70 g	91.6	24	-4
Cannabis 200 mg	74.1	19	-2
Cannabis 300 mg	73.0	51*	-3
Cannabis 400 mg	75.6	54*	-11

Average time spent on passing through the most complex out of four finger labyrinths. Change (%) indicates differences between drug test or post-test and pre-test values. Top half is accumulated data from parts I and II (cannabis versus alcohol), bottom half from part III (dose-response relationship for cannabis).

* $P < 0.05$.

There was no significant change in digit span after either cannabis or alcohol. On the addition tests, 200 mg of cannabis resulted in a 7% increase in time taken, 300 mg, a 37% increase, and 400 mg, a 56% increase (Table 1). The differences in errors were not statistically significant either after cannabis or after alcohol. A similar pattern was seen in serial subtraction of sevens (Table 2) with time increases after cannabis of 9%, 21%, and 57%. The error score did not change after alcohol and showed only slight increase after cannabis. In the finger labyrinth test (Table 3) both time and error scores were generally more influenced by the drug, the more complex the labyrinths and instructions. Cannabis at 300 and 400 mg increased time scores in the three more difficult labyrinths, alcohol having a similar but in this case not statistically

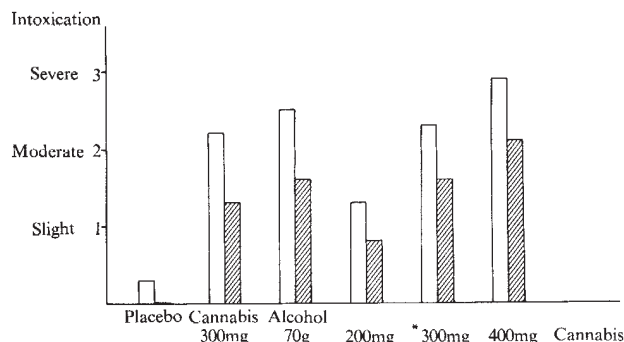


Fig. 1 Rating by subjects (white columns) and by experimenters (hatched columns) of degree of intoxication midway during drug test (~105 min after cannabis and 75 min after alcohol administration). Cannabis contained 4% Δ^1 -THC. Average from eight subjects. * Values for seven subjects. Left half is accumulated data from part I and part II (cannabis versus alcohol), right half from part III (dose-response relationship for cannabis).

significant effect. The effect of cannabis on errors was more marked than that of 70 g alcohol. There were irregular dose-response trends for cannabis both on time scores and on errors. Sustained attention (Bourdon's cancellation test) was not influenced by cannabis or by alcohol as measured by number of scanned letters. A slight increase in errors was seen after alcohol.

In the Smith and Beecher's subjective questionnaire cannabis and alcohol produced consistent changes on the category "mental clouding", including the three dimensions: alert/mind slow, dreamy/not dreamy, clear-headed/groggy. For alcohol, statistical significance was obtained on the clear-headed/groggy dimension, for cannabis statistical significance was obtained on all three dimensions. The changes were in the direction of "mind slow", "dreamy", and "groggy". On the category "inactivity", including the dimensions sleepy/awake and peppy/no-pep there was a tendency for the subjects to rate themselves more sleepy and less peppy after cannabis. There was a dose-response relationship for the dimension peppy/no-pep.

On the clear-headed/groggy dimension, the experimenters' ratings of the effects of both alcohol and cannabis were statistically significant for all treatments except for the smallest dose of cannabis 200 mg. Sleepiness seemed similarly strongly related to cannabis and, to a lesser extent, lack of motivation and incoherent speech. The dimensions clear-headed/groggy, talkative/silent, and high spirits/low spirits were influenced by alcohol to a statistically significant degree. The changes were in the direction "groggy", "talkative", and "high spirits". Combined results of the subjective and objective rating of intoxication (Fig. 1) showed that in general the subjects rated themselves more intoxicated than did the experimenters. Otherwise the result showed strong agreement between subjects and experimenters: subjectively and objectively six out of eight subjects were rated intoxicated by cannabis after 200 mg, and all eight subjects were rated intoxicated by cannabis after 400 mg; after 70 g alcohol all eight subjects were rated intoxicated by alcohol. Two subjects rated themselves intoxicated by cannabis after placebo and one subject was rated intoxicated by cannabis after placebo by the experimenter.

The results obtained at post-test the following morning are not described in detail because the post-test values never differed to a statistically significant extent from the pre-test values (as seen in the tables).

Most earlier experiments have failed to show a relationship between subjective experience of cannabis intoxication and measurable parameters such as visual⁴⁻⁷ and time^{8,9} perception, speech difficulties¹⁰, psychomotor function^{4,5}, sustained attention¹¹, and memory span^{6,12,13}. Measurable effects have, however, been found on more complex tasks such as reaction time tests involving choice or discrimination, tasks

involving fine coordination¹¹ some memory tests¹⁴, and other intellectual tasks demanding the retention and coordination of information¹⁵.

Our results¹⁶ are in general agreement with the earlier findings, and we feel that the differential effect of the drug on time and error scores is important. This may be related to the loss of items from short term memory, while long term memory is relatively unaffected; one of our subjects said, "The events of the preceding seconds are lost, but if I wait for a short while, they pop up again".

The large day-to-day variations in the effect of cannabis showed no consistent trend (Tables 2 and 3), and were probably due to a combination of normal physiological variations and the characteristic waxing and waning of cannabis intoxication. The effects of cannabis can clearly be shown by cognitive tests, though the specific character of the intoxication is more apparent phenomenologically; neither subjects nor experimenters were ever in doubt about which drug had been administered, except in a few instances with the smallest dose of cannabis.

We thank the Danish Ministry of the Interior, the National Health Service of Denmark, and the Danish Civil Defence Corps for permission to conduct this study and to seek volunteers among conscript personnel; Ebbe Linnemann, Poul Thygesen, Gudmund Magnussen and their staffs for ward and laboratory facilities; Lars von der Lieth for analysis of the MMPI questionnaires; Bent Kofod for technical help; and the United Nations Division of Narcotic Drugs, Geneva, for cannabis and THC. The research was supported in part by grants from *Forsikringsselskabernes Fond* and the Danish Medical Research Council.

LISE RAFAELSEN
HENRIETTE CHRISTRUP
PER BECH
OLE J. RAFAELSEN

Department of Psychiatry,
Gentofte County Hospital,
DK-2900 Hellerup, and
Psychochemistry Institute,
Rigshospitalet,
9 Blegdamsvej,
DK-2100 Copenhagen

Received July 5; revised November 27, 1972.

- Rafaelsen, O. J., Bech, P., Christiansen, J., Christrup, Henriette, Nyboe, J., and Rafaelsen, Lise, *Science* (in the press).
- Smith, G. M., and Beecher, H. K., *J. Pharmacol. Exp. Ther.*, **126**, 50 (1959).
- Siegel, S., *Non-parametric Statistics* (McGraw-Hill, New York, 1956).
- Morrow, R. S., in *Mayor La Guardia's Committee on Marihuana, The Marihuana Problem in the City of New York—Sociological, Medical, Psychological, and Pharmacological Studies* (Cattell, Lancaster, 1944).
- Clark, L. D., and Nakashima, E. N., *Amer. J. Psychiat.*, **125**, 379 (1968).
- Williams, E. G., Himmelsbach, C. K., Wikler, A., Ruble, D. C., and Lloidy, B. J., *Publ. Hlth. Rep. (Wash.)*, **61**, 1059 (1946).
- Caldwell, D. F., Myers, S. A., Domino, E. F., and Merriam, P. E., *Percept. Motor Skills*, **29**, 755 (1969).
- Dornbush, R. L., Fink, M., and Fredman, A. M., *Amer. J. Psychiat.*, **128**, 194 (1971).
- Jones, R. T., and Stone, G. C., *Psychopharmacologia*, **18**, 108 (1970).
- Weil, A. T., and Zinberg, N. E., *Nature*, **222**, 434 (1969).
- Weil, A. T., Zinberg, N. E., and Nelsen, J. M., *Science*, **162**, 1234 (1968).
- Halpern, Florence, in *Mayor La Guardia's Committee on Marihuana, The Marihuana Problem in the City of New York—Sociological, Medical, Psychological, and Pharmacological Studies* (Cattell, Lancaster, 1944).
- Waskow, I. E., Olsson, J. E., Salzman, C., and Katz, M. M., *Arch. Gen. Psychiat.*, **22**, 97 (1970).
- Abel, E. L., *Nature*, **227**, 1151 (1970).
- Melges, F. T., Tinklenberg, J. R., Hollister, L. E., and Gillespie, H. K., *Science*, **168**, 1118 (1970).
- Rafaelsen, O. J., Bech, P., and Rafaelsen, Lise, *Intern. Pharmacopsychiat.* (in the press).

Long-term Interactions of Marijuana and Behaviour in Chimpanzees

MARIJUANA produces differential effects in human naive and experienced marijuana smokers¹, but owing to ethical constraints involved in repeatedly administering marijuana to drug-naive humans, many human experiments have involved only experienced marijuana users as subjects. These constraints, however, are minimal with non-human subjects, and the great majority of animals used in marijuana research have been drug naive. So far, distinguishing between the long-term effects of marijuana in drug-naive and drug-experienced animal subjects has little precedence in the literature. We report here a number of observations, made over a two-year period during our marijuana research with chimpanzees, which indicate that experiential factors may play a major role in the effects of marijuana on complex behaviour.

Most of our recent marijuana-behaviour studies have been performed with a delayed matching to sample task. The experimental paradigm involves presenting one of six sample stimuli on a translucent key mounted on a stimulus-response panel fixed to the chimpanzee's cage. The chimpanzee must press this sample key ten times when the sample stimulus goes out and a delay period is initiated. After the delay a different stimulus is presented on each of the three choice keys mounted below the sample key, one of which is the same as that previously presented on the sample key. If the chimpanzee chooses this stimulus, a 1 g banana-flavoured pellet is delivered into a foodwell located below the keys and the trial ends. If the chimpanzee chooses either of the two incorrect stimuli, the trial is simply terminated and another begun after a 15 s intertrial interval. The stimuli used are three forms ($\times$, —, Δ) and three colours (red, blue and green); within a trial all are from the same stimulus dimension, approximately half are colour trials and the other half are form trials. Generally, each chimpanzee completes 100 trials per day. The five adult chimpanzees used in the experiments reported here had previously been administered several doses of a marijuana compound while performing under simple operant schedules of reinforcement^{2,3}.

Our previous data indicated that the delayed matching-to-sample performance of chimpanzees was disrupted by single marijuana administrations⁴⁻⁶. Our first delayed matching-to-sample observations were made during a four month period while running the chimpanzees on various delay values (for example, 0, 5, 20, and 40 s) of the matching task, and assessing the effects of 1.0 mg kg⁻¹ Δ^9 -trans-tetrahydrocannabinol (Δ^9 -THC), the major active ingredient of marijuana, on performance at each delay^{4,5}. Subsequently, the chimpanzees were maintained for an additional year on the delayed matching-to-sample task, primarily at a 20 s delay, during which time they experienced over 80 additional drug doses ranging from 1.0 to 4.0 mg kg⁻¹ Δ^9 -THC⁶. Although the behaviour in the first phase of our delayed matching-to-sample observations was asymptotically stable during the time period studied, by the end of the subsequent year the nondrug performance had improved considerably.

To determine whether the original relationship between the disruptive effects of Δ^9 -THC and the length of the matching-to-sample delay interval had been maintained across the extensive exposure to the behavioural task and drug, the initial delayed matching-to-sample experiment was systematically replicated with the same chimpanzees. In this second phase only delays of 5, 20, and 40 s were used. The basic procedure for both phases involved running the chimpanzees at each delay for a number of consecutive days until the behaviour had stabilized, after which 1.0 mg kg⁻¹ Δ^9 -THC was administered orally in a vehicle of corn syrup, orange extract, and water. On nondrug days they received the drug vehicle alone; both drug and nondrug doses were administered 2.5 h before the matching-to-sample session.

The mean percentage of correct delayed matching-to-sample responses obtained during each experimental phase is plotted

in Fig. 1. A comparison of the nondrug control data for the two phases clearly indicates that the accuracy of matching responses had improved during the chimpanzee's year-long exposure to the delayed matching task. In both phases, matching accuracy decreased as the length of the delay interval was increased. This inverse relationship was significant for both phase 1 ($F=46.6$; $d.f.=3, 12$; $P<0.01$) and phase 2 ($F=10.5$; $d.f.=2, 8$; $P<0.01$); however, Fig. 1 indicates that the delay values at which Δ^9 -THC produced a significant impairment in matching accuracy had changed between phases 1 and 2.

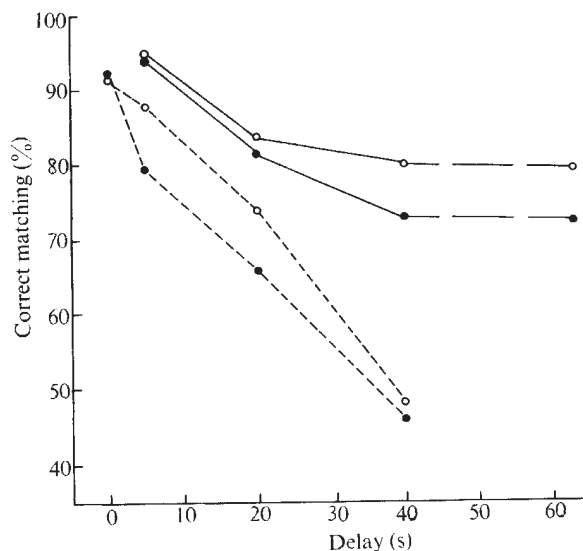


Fig. 1 Mean percentage of correct delayed matching-to-sample responses obtained during nondrug control (O) and 1.0 mg kg⁻¹ Δ^9 -THC drug (●) sessions at several delay values. Data for phase 1 (---) were obtained approximately one year before the data for phase 2 (—). Data points for the 63 s delay were obtained shortly after phase 2.

Multiple *t*-tests for the first phase of our observations showed that only the drug effects obtained at the 5 and 20 s delays were significant relative to nondrug control responding (respective t s = 3.9 and 3.4; $d.f.=4$; $P<0.05$). The absence of a Δ^9 -THC effect on the 0 s delay was observed by Zimmerberg *et al.*⁷ and may be interpreted to mean that the drug-produced impairments in matching accuracy were not primarily due to losses of attention or to distortions in perceiving the stimuli presented. Instead, the effects of Δ^9 -THC would seem to be specific to that aspect of the delayed matching task which involves short-term memory. Lack of a drug effect at the 40 s delay may have been due to the fact that nondrug matching accuracy at this delay value approached chance performance (33% correct) for some chimpanzees. In contrast, in phase 2 of the experiment, little or no drug effect was observed at 5 and 20 s delays and only the drug effect obtained at the 40 s delay was statistically significant ($t=2.8$; $d.f.=4$; $P<0.05$).

Drug effects obtained during phase 1 at 5 and 20 s delays were not replicated during phase 2 which suggests that tolerance developed to the effects of Δ^9 -THC on matching performance at these delays. Whether this tolerance was due to the additional behavioural training received by the chimpanzees during the intervening year or to the fact that they had become more experienced with Δ^9 -THC, or both, cannot easily be determined from this experiment. One interpretation is that higher levels of matching accuracy are not as readily impaired by Δ^9 -THC as are lower performance levels, and data obtained at the 0 s delay in phase 1 and at the 5 and 20 s delays in phase 2 are consistent with this. That is, matching behaviour at these

delays may have been so well learned that it was not susceptible to the disruptive effects of the drug; however, as can be seen in Fig. 1, nondrug matching accuracy at the 5 s delay in phase 1 was somewhat higher than it was for the 20 s delay in phase 2 and a significant drug effect was obtained for only the former condition. On the other hand, in a test run between phases 1 and 2 of the present experiment⁶, there was no indication of tolerance development to Δ^9 -THC when chimpanzees were dosed for 21 or 42 consecutive daily sessions under the same 20 s delayed matching-to-sample task.

Observations suggested that the intermixing of drug and nondrug experiences, as well as the number of drug experiences, may be important to the development of Δ^9 -THC tolerance. Subsequent to phase 2, animals were maintained at a 40 s delay value and were again administered 1.0 mg kg⁻¹ Δ^9 -THC on two occasions separated by several nondrug days. The last of these drug administrations produced a minimal decrease in the percentage of correct matching responses from 80.1% (nondrug control) to 77.5% (drug). This latter drug-produced decrease in matching accuracy did not reach statistical significance ($t=0.97$; $d.f.=4$; $P>0.10$), thus again demonstrating tolerance to Δ^9 -THC, in this instance at the long matching-to-sample delay of 40 s. As this latter tolerance had developed quite rapidly, we attempted to determine whether the drug was still behaviourally inactive by increasing the length of the delay interval still further to 63 s. The chimpanzee's nondrug performance at this delay quickly stabilized at approximately 80% correct matching, under a 1.0 mg kg⁻¹ dose of Δ^9 -THC, matching accuracy was significantly decreased to 72.7% ($t=2.8$; $d.f.=4$; $P<0.05$). The nondrug control and drug data for the 63 s delay have been included in Fig. 1 as part of phase 2.

A definite pattern of long-term marijuana-behaviour interactions had been established; reliable Δ^9 -THC effect was first produced at a short matching-to-sample delay value; continued intermixing of nondrug and drug sessions at that delay value was accompanied by a reduction in the drug effect. By increasing the length of the delay, a statistically reliable drug effect was again obtained but diminished after the chimpanzees had experienced a number of nondrug to drug transitions at that delay, and so on. It is difficult to offer a pharmacological explanation for our observations that tolerance developed to Δ^9 -THC effects at a particular delay value but did not persist when the length of the delay was increased. Indeed, it seems more appropriate to suggest that the observed tolerance was the result of the chimpanzee's learning to perform behaviour which compensated for the drug effects produced in a particular delay situation. Repeated exposures to a defined delayed matching-to-sample task under mixed nondrug and drug conditions may provide the chimpanzee with sufficient opportunities to acquire responses which counteract the detrimental behavioural effects produced by Δ^9 -THC in that specific delay situation⁸.

Although the nature of these learned compensatory responses cannot now be specified, a comparable adjustment process may occur in human marijuana smokers. In general, the performance of experienced human marijuana users on behavioural tasks is not as susceptible to the disruptive effects of marijuana as is that of drug-naïve humans even though the subjective effects of marijuana may become more intense with repeated drug experiences¹. Nevertheless, there are several implications of the present hypothesis which are in need of direct empirical verification. Most importantly, it would seem that once compensatory responses had been learned in a specific stimulus situation, tolerance to drug produced behavioural effects in that situation should be relatively permanent. That is, at least a partial tolerance should be retained even after a long-term abstinence from both the drug and the stimulus situation had been enforced, but no data for Δ^9 -THC are available, even of an indirect nature, which support this implication.

This work was supported by a research contract from the US National Institute of Mental Health to D. P. F. We thank

Dr Monique Braude for the Δ^9 -THC and advice, and D. Billings and M. Grisham for their assistance.

D. M. GRILLY
D. P. FERRARO
R. G. MARRIOTT

Department of Psychology,
University of New Mexico,
Albuquerque, New Mexico 87106

Received October 13; revised December 5, 1972.

- ¹ Secretary, Department of Health, Education and Welfare, *A Report to the Congress: Marijuana and Health* (US Government Printing Office, Washington, 1971, 1972).
- ² Ferraro, D. P., Grilly, D. M., and Lynch, W. C., *Psychopharmacologia*, **22**, 333 (1971).
- ³ Ferraro, D. P., Lynch, W. C., and Grilly, D. M., *Pharmacology* (in the press).
- ⁴ Ferraro, D. P., *Fifth International Congress on Pharmacology*, San Francisco, California (1972).
- ⁵ Ferraro, D. P., *Current Research in Marijuana* (edit. by Lewis, M. L.), 50 (Academic Press, New York, in the press).
- ⁶ Ferraro, D. P., and Grilly, D. M., *Science* (in the press).
- ⁷ Zimmerberg, B., Glick, S. D., and Jarvik, M. E., *Nature*, **233**, 343 (1971).
- ⁸ Dews, P. B., *Experimental Foundations of Clinical Psychology* (edit. by Bachrach, A. J.), 423 (Basic Books, New York, 1962).

T-mycoplasmas as a Possible Cause for Reproductive Failure

T-MYCOPLASMAS have been found associated with low birth weight¹ and have been isolated from the foetal membranes in cases with repeated spontaneous abortions²⁻⁴. It has been suggested, therefore, that T-mycoplasmas might be important for human reproductive failure. Recently, these findings have been supported by Caspi *et al.*⁵ who reported a case with amnionitis due to T-mycoplasmas.

We have investigated the possible role of mycoplasmas in human reproductive failure in patients with primary sterility. Before the patients were included in the study they were subjected to a careful clinical examination. All females had normal pelvic examinations, biphasic basal body temperature curves, no signs of a deficient luteal phase (as examined by plasma progesterone values and an endometrial biopsy). All had regular pelvic roentgenological findings. All males had more than 20 million sperm cells per ml., less than 40% morphologically abnormal forms and a good sperm motility in four ejaculates examined within a period of 12 weeks. A total of fifty-two married couples with a primary sterility of 5 yr duration or more were included in the study. Two ejaculates and three specimens of cervical mucus were obtained from each couple and cultured for mycoplasmas with standardized techniques^{6,7}. The results obtained were compared with the results of two control groups with proven fertility; cervical cultures from pregnant females attending the ante-partum care clinic (group II) and ejaculates from males, married to pregnant or recently delivered women (group III).

T-mycoplasmas were isolated from ninety-two of the 104 patients in group I (89%) (Table 1). T-mycoplasmas were

Table 1 Growth of T-mycoplasmas in Cervical Mucus and Seminal Fluid in Infertile (I) and Fertile (II, III) Patients

Group number	I		II	III
Sex	♂	♀	♀	♂
Growth of T-mycoplasmas	45	47	9	6
No growth	7	5	31	17
Total number of patients	52	52	40	23

isolated from the cervix of nine of the forty pregnant women in group II (23%) and from the ejaculates of six of the twenty-three men in group III (26%). "Conventional" mycoplasmas, mostly *M. hominis*, were isolated only occasionally from cervical mucus or ejaculates. The difference in incidence of T-mycoplasmas between infertile (group I) and fertile patients (groups II and III) was found highly statistically significant ($P < 0.001$).

After an observation period of 3 months both husbands and wives in group I were treated with doxycycline in recommended dosage for 10 days between the seventh and the sixteenth day of the menstrual cycle in order to eradicate the T-mycoplasmas and to evaluate if this had any influence on the patients with regard to their fertility status or not. If no pregnancy ensued the treatment was repeated during the next two months at the same period of the menstrual cycle. During the fourth and fifth month the patients were treated with doxycycline in increased dosage: 200 mg daily from the seventh to the sixteenth day. If pregnancy was reported, no further treatment was given.

In the ten males and eleven females studied in detail the concentrations of doxycycline in serum varied between 0.4 and $1.35 \mu\text{g ml}^{-1}$ 12–14 h after the last dose of doxycycline, while those in ejaculates varied between 0.2 and $1.8 \mu\text{g ml}^{-1}$ (mean value $0.95 \mu\text{g ml}^{-1}$). Most of the isolated T-mycoplasmas were inhibited by doxycycline in concentrations below $0.1 \mu\text{g ml}^{-1}$ although some of the isolates had MICs up to $0.6 \mu\text{g ml}^{-1}$. The T-mycoplasmas disappeared from all ten males and from all but two of the eleven women studied in detail.

The patients were observed carefully for 5 months. After 3 months pregnancies had been reported in twelve couples. After 5 months another three pregnancies were reported.

It is suggestive that 29% of the women in group I became pregnant within a few months after the eradication of their T-mycoplasmas, after their earlier longstanding infertility. The results of this investigation therefore support the suggestion that at least some T-mycoplasmas may be of importance in some cases with reproductive failure where no other cause is detected.

These findings are now being further investigated.

HÅKAN GNARPE

*Institute of Medical Microbiology,
University of Uppsala*

JAN FRIBERG

*Department of Obstetrics and Gynaecology,
University Hospital,
Uppsala*

Received October 30; revised November 24, 1972.

- ¹ Braun, P., Lee, Y.-H., Klein, J. O., Marcy, M., Klein, T. A., Charles, D., Levy, P., and Kass, E. H., *New Engl. J. Med.*, **284**, 167 (1971).
- ² Kundsinn, R. B., Driscoll, S. G., and Ming, P.-M., *Science*, **157**, 1573 (1967).
- ³ Kundsinn, R. B., and Driscoll, S. G., *Ann. NY Acad. Sci.*, **174**, 794 (1970).
- ⁴ Kundsinn, R. B., and Driscoll, S. G., *Surg. Gynecol. Obstet.*, **131**, 89 (1970).
- ⁵ Caspi, E., Herczeg, E., Solomon, F., and Sompolsky, D., *Amer. J. Obstet. Gynecol.*, **111**, 1102 (1971).
- ⁶ Chanock, R. M., Hayflick, L., and Barlie, M. F., *Proc. US Nat. Acad. Sci.*, **48**, 41 (1962).
- ⁷ Shepard, M. C., and Luncford, D. D., *Appl. Microbiol.*, **20**, 539 (1970).

Human Lymphocyte Antigen Association in Ankylosing Spondylitis

A STUDY of human lymphocyte phenotypes in unrelated Caucasian individuals with ankylosing spondylitis has revealed a striking similarity in their antigenic pattern. The lympho-

cytes of fifty such patients, when tested against a panel of twenty-six different specific typing sera, using a two stage lymphocytotoxicity micro-method, were shown to have in common either the antigen HL-A27 (96%) or the antigen W5 (4%). This remarkably high frequency of the antigen HL-A27 compares with the incidence of 5–6% of this antigen in random Caucasian populations.

Antigen HL-A27 has been shown to have a higher frequency among certain "isolated" communities such as the Pima Indians (10%), (ref. 1). Among these Indians, there is a higher incidence of ankylosing spondylitis than in a random Caucasian population², being 5.9% compared with 0.5% or less in the latter group. Likewise, there is a low frequency of this antigen among Negroid populations (0–1%) among whom there is a very low incidence of this disease³. These findings support the observation above, details of which will be reported later.

Significant associations between the human lymphocyte phenotype and groups of patients with various diseases involving the lymphatic system have been reported by some authors. The antigens HL-A5, W5 and W18 are found more frequently in patients with Hodgkin's disease^{4–6}. Systemic lupus erythematosus is associated with HL-A8 and W15^{7,8}, adult coeliac disease with HL-A1 and HL-A8⁹ and HL-A13 and possibly W17 with psoriasis¹⁰. HL-A9 and HL-A12 are more frequent in patients with chronic glomerulo-nephritis¹¹. The results of two separate studies of patients with rheumatoid arthritis were, however, conflicting and inconclusive^{12,13}. An increased frequency of HL-A27 (17%) has been noted in patients with lymphoblastic leukaemia¹⁴. This is perhaps of relevance to ankylosing spondylitis patients as some of them did develop leukaemia after radiotherapy.

It appears therefore that various HL-A phenotypes are associated with certain diseases. We also postulate that ankylosing spondylitis may be mediated partly by histocompatibility influenced immune responses.

We thank Drs Brewerton and Hart and their patients, and their registrars, Drs A. Nicholls and R. Sturrock. M. F. P. C. thanks the Governors of Westminster Hospital for a grant.

MAEVE F. P. CAFFREY
D. C. O. JAMES

*Tissue Typing Laboratory,
Blood Transfusion Unit,
Westminster Hospital,
London SW1*

Received January 12, 1973.

- ¹ Amos, D. B., *Fifth International Histocompatibility Testing Workshop*, Evian, May 1972.
- ² Gofton, J. P., Bennett, P. H., Smythe, H. A., and Decker, J. L., *Ann. Rheum. Dis.*, **31**, 474 (1972).
- ³ Baum, J., and Ziff, M., *Arthritis Rheumatism*, **13**, 305 A (1970).
- ⁴ Amiel, J. L., *Histocompatibility Testing 1967*, 79 (Munksgaard, Copenhagen, 1967).
- ⁵ Zervas, J. D., Delamore, I. W., and Israels, M. C. G., *Lancet*, ii, 634 (1970).
- ⁶ Falk, J., and Osoba, D., *Lancet*, ii, 1118 (1971).
- ⁷ McDavitt, H. O., and Bodmer, W. F., *Amer. J. Med.*, **52**, 1 (1972).
- ⁸ Walford, R. L., Smith, G. S., and Waters, H., *Transplant. Rev.*, **7**, 78 (1971).
- ⁹ Stokes, P. L., Asquith, P., Holmes, G. K. T., Mackintosh, P., and Cooke, W. T., *Lancet*, ii, 162 (1972).
- ¹⁰ Russell, T. J., Lorain, M. D., Schults, M., and Kuban, D. J., *New England J. Med.*, **287**, 738 (1972).
- ¹¹ Mickey, M. R., Kreisler, M., and Terasaki, P. I., *Histocompatibility Testing 1970*, 237 (Munksgaard, Copenhagen, 1970).
- ¹² Kueppers, F., Brackertz, D., and Mueller-Eckhardt, C. H., *Lancet*, ii, 1425 (1972).
- ¹³ Seignalet, J., Clot, J., Sany, J., and Serre, H., *Vox Sanguinis*, **23**, 468 (1972).
- ¹⁴ Lawler, S. D., and Klouda, P. T., *Brit. J. Haematol.*, **21**, 595 (1971).

High Affinity Transport of 2-Deoxyglucose in Isolated Synaptic Nerve Endings

RESULTS of studies of transport in the nervous system using whole brain and slice preparations suffer from the disadvantage that it is not certain whether they apply to capillary, glial, and/or neuronal cells. Isolation of intact synaptic nerve endings (synaptosomes) has made it possible to investigate transport across synaptic membranes which are free of significant glial contamination¹. Synaptosomes can respire and generate ATP and phosphocreatine with glucose as substrate, and glucose metabolism is largely abolished when synaptosomes are ruptured². The data reported here suggest that carrier mediated sugar transport across synaptic membranes has different characteristics from that seen in brain and brain slices.

Synaptosomes were prepared from rat brain^{1,3}. They were inspected by electron microscopy by Dr J. R. Baringer of this department and resembled synaptosomal fractions in published reports. The standard conditions of incubation and assay in all experiments were as follows. Aliquots of the synaptosomal suspension (0.1 ml.) were incubated in triplicate for 15 min at 37° C in 0.5 ml. of a solution (pH 7.4) containing 264 mM sucrose, 26 mM potassium phosphate buffer, and carrying concentrations of ³H-2-deoxyglucose (³H-2-DG) or sucrose. Uptake of radioactive 2-DG into synaptosomes was stopped by adding 5 ml. of 0.32 M ice-cold sucrose and the contents immediately filtered and washed with sucrose on 0.45 µm 'Millipore' filters; the filters were transferred directly to counting vials containing a triton-toluene scintillation solution for determination of radioactivity retained in the synaptosomes³. The assay was linear in time for at least 20 min and with increasing protein concentration up to at least 0.8 mg protein. In blank tubes designed to measure non-specific entry of radioactive sugar, 200 mM non-radioactive glucose replaced an equal concentration of sucrose prior to addition of synaptosomes. The large excess of non-radioactive glucose is assumed to prevent specific uptake of ³H-2-DG but not effect diffusion³. Radioactivity in synaptosomes in blank tubes was reduced to less than 5% of the total. 2-DG is metabolized only to 2-deoxyglucose-6-phosphate (2-DG-PO₄)⁴. Separation of 2-DG from 2-DG-PO₄ was carried out by a two step chromatographic procedure using a 2.5×0.5 cm column containing 'Dowex 1-X8', 100–200 mesh. 2-DG is not retained by the column and is eluted with water; 2-DG-PO₄ is eluted with 1.4 N HCl.

Rat brain synaptosomes incubated with ³H-2-DG were assayed for uptake by 'Millipore' filtration. Nonspecific entry of radioactivity into synaptosomes increased linearly with substrate concentration and this minor component was subtracted from values obtained with the active system. Specific uptake of radioactivity exhibited saturation kinetics with increasing 2-DG concentration. K_m , the apparent concentration for half-maximal uptake of 2-DG into synaptosomes, was 0.24 mM (Fig. 1). This is 42 to 375 times less than values obtained in brain slices (10–90 mM)^{5–9} and indicates that nerve endings have a highly specific transport system for glucose. 2-DG uptake per mg protein by microsomes, mitochondria and synaptosomes ruptured by osmotic lysis or sonication was 4–9% of intact synaptosomes. Insulin (0.02 U ml.⁻¹) was without effect on 2-DG uptake whether synaptosomes were prepared from normal rats or animals made diabetic with streptozotocin¹⁰. After incubation with 1 mM 2-DG under standard conditions ($n=8$), 23% of the radioactivity taken up by synaptosomes was recovered as free 2-DG and 77% ± 3.4 s.d. as 2-DG-PO₄. Since glucose-6-phosphatase activity is not prominent in the nervous system¹¹ the presence of free 2-DG inside synaptosomes suggests that 2-DG entered without phosphorylation. Subsequent

phosphorylation of 2-DG, however, might be rate limiting and the K_m for 2-DG uptake in synaptosomes might be a result of hexokinase activity. To determine whether transport or phosphorylation was rate limiting we measured the percentage of free 2-DG and 2-DG-PO₄ recovered in synaptosomes after incubation with 1 µM to 5 mM 2-DG ($n=26$). If phosphorylation was rate limiting, the percentage of free 2-DG should increase with increasing concentrations of substrate above the K_m for uptake. Instead, we found that the percentage of radioactivity recovered as free 2-DG remained the same despite a 5,000-fold range of substrate concentration. The average was 75.3% ± 4.7 s.d. This indicates that phosphorylation of 2-DG is linear with total uptake of 2-DG whereas total uptake of 2-DG is saturable. The K_m for 2-DG uptake in these experiments therefore seems to be determined by transport of 2-DG across the synaptosomal membrane and not its subsequent phosphorylation. Studies carried out with 3-O-methylglucose (3-MG) further support this interpretation.

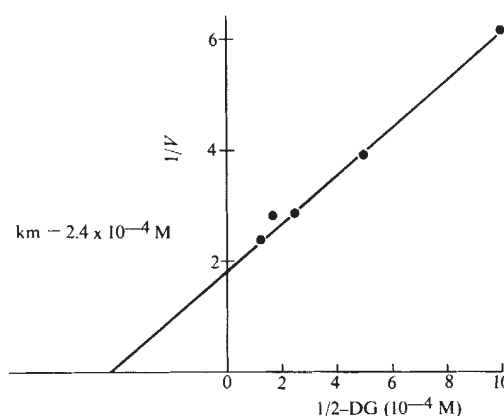


Fig. 1 A Lineweaver and Burke plot of ³H-2-deoxyglucose uptake into synaptosomes containing 0.4 mg protein. The ³H-2-DG specific activity is 7.6×10^5 c.p.m. µmol⁻¹. $V = \text{c.p.m.} \times 10^{-4}$. After incubation with 10^{-4} M 2-DG, the intrasynaptosomal concentration was at least five times greater than the medium. Blank values ranged from 0.03% to 3.8% of the active system with increasing substrate concentration. Each point is the average of three determinations.

3-MG is a non-metabolizable sugar which is transported into brain¹² but does not react with hexokinase and is not phosphorylated⁴. Nevertheless, 3-MG and 2-DG should compete for entry into synaptosomes if they share a common carrier-mediated process. Incubation of synaptosomes with 3-MG depressed uptake of ³H-2-DG; 3-MG is a competitive inhibitor of ³H-2-DG uptake with a K_i of 6.5 mM (Fig. 2a). Even though 3-MG does not react with hexokinase, 3-MG and 2-DG seem to have highly specific affinities for a common glucose carrier in isolated nerve endings.

Phloretin inhibits glucose transport in many tissues but has not been reported to inhibit transport in brain. In synaptosomes, however, phloretin is a competitive inhibitor of 2-DG uptake with a K_i of 0.75 µM (Fig. 2b). This value is probably far below that for inhibition of glucose metabolism¹³ and suggests that the drug reacts directly with the glucose carrier in nerve endings. By contrast, phlorizin did not inhibit 2-DG uptake at these concentrations; also NaCl (0.075 M), which replaced sucrose (0.15 M), and ouabain (0.1 mM) were without effect. These findings are consistent with the concept that phloretin tends to inhibit Na⁺ independent sugar transport¹⁴.

The demonstration of a high affinity glucose transport system is reminiscent of several high affinity uptake processes for amines and amino-acids in synaptosomes which have been described recently¹⁵. The presence of such high

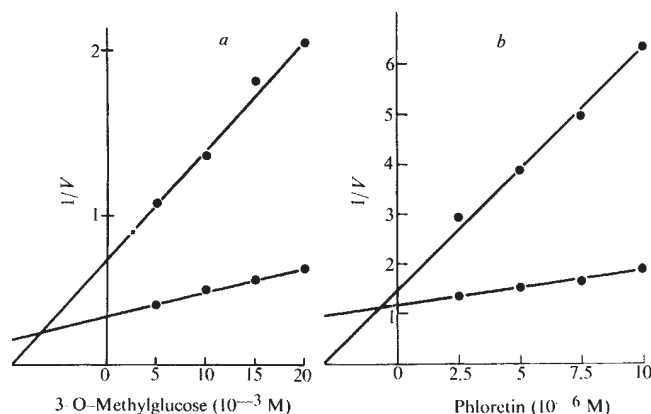


Fig. 2 Dixon plots of 3-O-methylglucose (a) and phloretin (b) inhibition of ^3H -2-DG uptake into synaptosomes containing 0.5 and 0.6 mg protein. The concentrations of ^3H -2-DG are 0.1 and 0.4 mM. The ^3H -2-DG specific activity is 10^7 c.p.m. μmol^{-1} and 1.8×10^6 c.p.m. μmol^{-1} . The K_i for 3-O-methylglucose and phloretin is 6.5×10^{-3} M and 7.5×10^{-7} M respectively. The conditions are as in Fig. 1.

affinity transport systems for metabolites in synaptosomes provides neurochemical evidence for specific utilization of these metabolites at the synapse. The affinity of 2-DG for transport in synaptosomes is much greater than in brain slices, and phloretin is a potent competitive inhibitor of this carrier-mediated glucose transport system. It is possible that nerve endings have a high affinity glucose transport system to support a high rate of presynaptic energy production. This would be consistent with the observation that dendritic layers in the cortex which contain both nerve terminals and dendrites seem to account for a high proportion of oxidative metabolism in grey matter¹⁶. On the other hand, synaptosomes (pinched-off nerve terminals) are enclosed by membranes derived from neurones and may reflect membrane properties of axons, neurones and dendrites. Consistent with the observation that neurones exhibit greater oxidative metabolism than glia¹⁷, it is also possible that a highly specific glucose transport system exists only in neuronal membranes. If true, the glucose transport system might be useful in helping to distinguish separated neurones from glial cells.

This work was supported by a grant from the US NINDS. We thank Professor I. Edelman for discussion and E. A. Bobier for technical assistance. I. D. is the recipient of a Research Scientist Career Development award from the US NINDS.

IVAN DIAMOND

Department of Neurology and Pediatrics,
University of California School of Medicine

ROBERT A. FISHMAN

Department of Neurology,
University of California School of Medicine,
San Francisco, California 94122

Received October 23; revised November 20, 1972.

- ¹ Gray, E. G., and Whittaker, V. P., *J. Anat.*, **96**, 79 (1962).
- ² Bradford, H. F., *J. Neurochem.*, **16**, 675 (1969).
- ³ Diamond, I., and Milfay, D., *J. Neurochem.*, **19**, 1899 (1972).
- ⁴ Sols, A., and Crane, R. K., *J. Biol. Chem.*, **210**, 581 (1954).
- ⁵ Joanny, P., Corriol, J., and Hillman, P., *Biochem. J.*, **112**, 367 (1969).
- ⁶ Bachelard, H. S., *J. Neurochem.*, **18**, 213 (1971).
- ⁷ Fishman, R. A., Reiner, M., and Diamond, I., *Neurology*, **21**, 414 (1971).
- ⁸ Diamond, I., and Fishman, R. A., *Neurology*, **21**, 414 (1971).
- ⁹ Cooke, W. J., and Robinson, J. D., *J. Neurochem.*, **18**, 1351 (1971).

- ¹⁰ Junod, A., Lamber, A. E., Stauffacher, W., and Renold, A., *J. Clin. Invest.*, **48**, 2129 (1969).
- ¹¹ Balázs, R., in *Handbook of Neurochemistry* (edit. by Lajtha, A.), **3**, 1 (Plenum Press, New York, 1970).
- ¹² Bidder, I. G., *J. Neurochem.*, **15**, 867 (1968).
- ¹³ Smyth, D. H., in *Membrane Transport and Metabolism* (edit. by Kleinzeller and Kotyk, A.), 488 (Academic Press, New York, 1961).
- ¹⁴ Stein, W. D., *The Movement of Molecules Across Cell Membranes*, 283 (Academic Press, New York, 1967).
- ¹⁵ Logan, W. L., and Snyder, S. H., *Nature*, **234**, 297 (1971).
- ¹⁶ Lowry, O. H., Robert, N. R., Leiner, K. Y., Wu, M. L., Farr, A. L., and Albers, R. W., *J. Biol. Chem.*, **207**, 39 (1954).
- ¹⁷ Rose, S. P. R., in *Applied Neurochemistry* (edit. by Davison and Dobbing, J.), 332 (F. A. Davis, Philadelphia, 1968).

Phytogeography of the Fungal Genus *Lycoperdon* in Relation to the Opening of the Atlantic

In the preparation of a world monograph of the genus *Lycoperdon* (Gasteromycetes) I have examined 8,500 herbarium collections of these puffballs from Europe (only the Baltic region in the case of the USSR) and North America. Abundance of material enabled grid maps to be drawn showing species distribution; such maps are rare in mycology owing to a lack of precise chorological information¹⁻³. Fungi were therefore seldom considered biogeographically⁴⁻⁷ until Kreisel's monograph of *Bovista*⁸ in 1967.

Map comparisons showed that *Lycoperdon* species presented well marked distribution types that could be classified into chorological elements comparable with those used for vascular plants (Fig. 1). Among these elements three groups could be distinguished; a subcosmopolitan element (two species); elements with a northern affinity (thirteen species); and those with a southern affinity (fifteen species). Most species from the second group (northern affinity) occur on both sides of the Atlantic while those with southern affinities are, with a single exception, unilateral and are phylogenetically more primitive on purely morphological grounds^{9,10}. Endemism is more developed and there are also three European/Eastern American vicarious pairs: *L. atropurpureum* Vitt. and *L. maurianum* Pat. ex Demoulin; *L. decipiens* Dur. et Mont and *L. rimulatum* Peck; and *L. echinatum* Pers. per Pers. and *L. americanum* Demoulin¹¹.

Three theories could explain the development in space and time of the genus. Recent long distance dispersal of spores over the Atlantic could explain the occurrence of similar species on both sides; vicarious pairs would be explained by an earlier crossing.

This does not, however, explain the restriction of some species to one continent, nor why those endemics and vicarious pairs only have southern affinities.

Long distance dispersal has been shown by many plant geographers to be overemphasized¹²⁻¹⁴. Crum¹⁵ showed that for Bryophytes it is limited to a few weed species; and I suggest that even for fungi and species such as *Lycoperdon* with wind dispersed spores of low terminal velocity and greater dispersal, oceans are still major geographical barriers. One of the reasons for this might be the loss of germinative ability observed on spores collected over the Atlantic¹⁶. It is usually considered that pathogenic fungi cannot cross oceans¹⁷⁻¹⁹ except possibly in the trade wind region²⁰.

If long distance dispersal is not the chief explanation of disjunct distributions, then alternatively somewhere in geological time there was an almost continuous terrestrial biota which favoured stepwise progression of species. Terrestrial migration through the Bering Straits, as for man and other late Cainozoic mammals, has been claimed, but various marine obstacles have existed in Siberia during a greater part of Mesozoic and Cainozoic²¹ and also the Bering area never seems to have been hospitable to warm temperate communi-

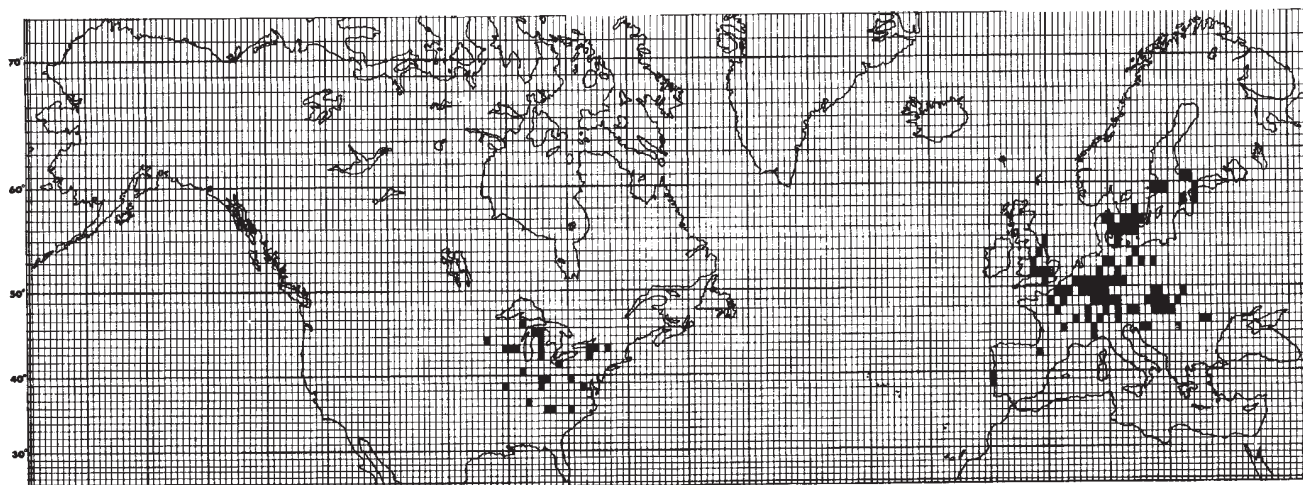


Fig. 1 Areas of two vicarious species, *Lycoperdon americanum* in America and *L. echinatum* in Europe.

ties²²⁻²⁴. Florin's maps of the past and present distribution of conifers²⁵ also argues against exchange of forest elements between Europe and America through Beringia.

Knowledge of plate tectonics²⁶⁻²⁹ enables the facts to be much better explained by continental drift than by any long distance or land-bridge theory, and it is now favoured by many biogeographers (refs. 30-41, and reviewed, ref. 42). In light of the drift theory, due to the more easterly position of the American plate, a Beringian land-bridge seems improbable before the Cainozoic⁴³, and many of the floral exchanges between Europe and North America seem to have occurred through Eastern America and Western Europe before the completion of the Atlantic barrier. This conclusion has already been reached by Axelrod³⁵ for the Mediterranean-South West USA floras.

Using plate tectonics data⁴³⁻⁴⁶ together with data on the history of climates²³ and floras^{22,47}, I conclude that since the late Cretaceous, first the North Atlantic, then the Labrador Sea must have started to interfere with the migration of subtropical and warm temperate floras. The junction of the

Norwegian Sea and the Arctic Ocean was not complete before the middle Eocene, allowing free interchange for cool temperate floras especially rich in conifers⁴⁸ (Fig. 2). The closing of the Norwegian Sea during the beginning of the Cainozoic is also supported independently of plate tectonics by sedimentology studies⁴⁹.

I consider that this explains why northern elements in *Lycoperdon* have not yet shown visible speciation between Europe and America, for not only did the populations remain in direct contact until the middle Eocene, but they were probably still able to exchange genes as long as Arctic regions remained habitable for them and seas were not too wide for easy crossing by wind dispersed spores. Southern elements, which have been separated for a much longer time, show vicariousness and endemism. The primitive morphological characters of the southern species and the richness and diversity of the southern flora suggest development of the genus in warm temperate broad-leaved deciduous forests⁵⁰ at a time that preceding considerations and parallelism with the development of modern forest types put as Cretaceous.

This would mean that fungi have evolved rather slowly since the Mesozoic. The fossil record is scanty but it is not in contradiction with this conclusion; the principal groups Phycmycetes s.l., Ascomycetes and Basidiomycetes were already in existence at the end of the Palaeozoic⁵¹⁻⁵⁴, polypores in the Jurassic⁵⁵, Palaeocene fungal spores with a very modern aspect⁵⁶, and Eocene spores attributed to the extant Gasteromycetous genus *Scleroderma*⁵⁷ have all been described.

Knowledge of Asiatic *Lycoperdon* is at present restricted owing to a lack of available data. The absence of a species related to *L. echinatum*-*L. americanum* in Eastern Asia should, however, be noted; it cannot be simply a consequence of lack of notice by collectors, as these fungi are two of the most striking of the genus. A study of Siberian material, which I hope to undertake, should give additional information on the theory described here. A palynological study of cores drilled at the level of the oldest magnetic anomalies at various parts of the North Atlantic and the Norwegian Sea should give biogeographers a picture of the environment of the northward opening ocean.

I thank D. M. Dring, R. P. Korf, J. Lambinon and J. T. Palmer for critical reading of my manuscript.

V. DEMOULIN*

Department of Botany,
University of Liège

Received August 9; revised December 4, 1972.

* Chargé de recherches du Fonds National Belge de la Recherche Scientifique.

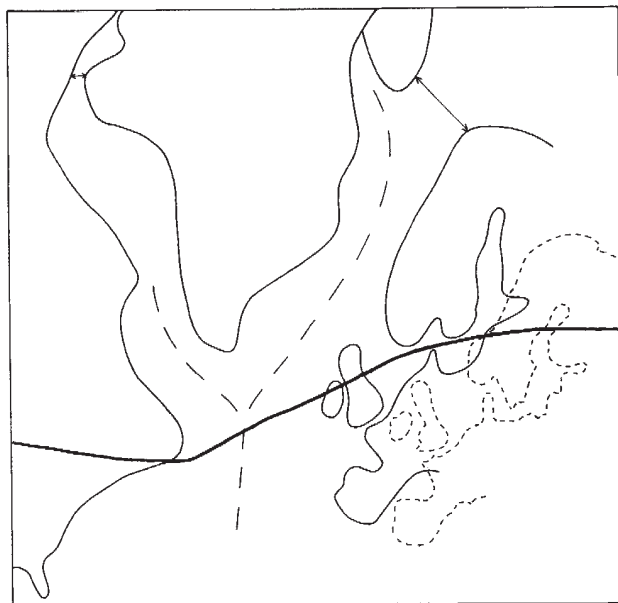


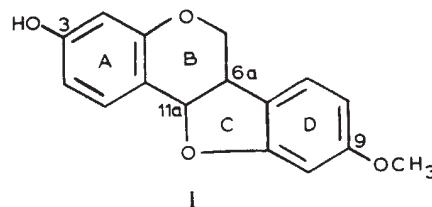
Fig. 2 Schematic position of actual land masses during the Eocene as interpreted from ref. 43. The thick black line is the limit between subtropical and temperate floras according to Takhtajan²². ---, Present position; —, oceanic barrier; ↔, possible terrestrial connexion.

- ¹ Hansen, L., and Lange, M., *Bot. Tidsskr.*, **62**, 46 (1966).
- ² Bresinsky, A., and Dichtel, B., *Z. Pflzk.*, **37**, 75 (1971).
- ³ Kreisel, H., *Feddes Repert.*, **82**, 589 (1971).
- ⁴ Bisby, G. R., *Amer. J. Bot.*, **20**, 246 (1933).
- ⁵ Diehl, W. W., *J. Wash. Acad. Sci.*, **27**, 244 (1937).
- ⁶ Overholts, L. O., *Mycologia*, **36**, 629 (1939).
- ⁷ Bisby, G. R., *Bot. Rev.*, **9**, 466 (1943).
- ⁸ Kreisel, H., *Taxonomisch-pflanzengeographische Monographie der Gattung Bovista* (Cramer, J., Lehre, 1967).
- ⁹ Hennig, W., *Phylogenetic Systematics* (University of Illinois, Urbana, 1966).
- ¹⁰ Wagner, jun., W. H., in *Systematic Biology*, 67 (US National Academy of Sciences Publication, 1962) (1969).
- ¹¹ Demoulin, V., *Lejeunia*, N.S., **61**, 1 (1972).
- ¹² Löve, A., and Löve, D., *Aquilo, Ser. Bot.*, **6**, 52 (1967).
- ¹³ van Steenis, C. G. G. J., *Blumea*, **11**, 235 (1962).
- ¹⁴ van Balgooy, M. M. J., *Blumea, Suppl.*, **6**, 1 (1971).
- ¹⁵ Crum, H., *J. Hattori Bot. Lab.*, **35**, 269 (1972).
- ¹⁶ Pady, S. M., and Kapica, L., *Mycologia*, **47**, 34 (1955).
- ¹⁷ Dupias, G., *Bull. Soc. Mycol. France*, **87**, 129 (1971-1972).
- ¹⁸ Ingold, C. T., *Fungal Spores, Their Liberation and Dispersal* (Clarendon, Oxford, 1971).
- ¹⁹ Ludlam, F. H., in *Airborne Microbes* (edit. by Gregory, P. H., and Monteith, J. L.), 1 (Soc. Gen. Microbiol., Seventeenth Symp., Cambridge, 1967).
- ²⁰ Bowden, J., Gregory, P. H., and Johnson, C. G., *Nature*, **229**, 500 (1971).
- ²¹ Kurtén, B., *Comment. Biol. Soc. Sci. Fennica*, **29**, 1 (1966).
- ²² Takhtajan, A., *Flowering Plants, Origin and Dispersal* (Oliver and Boyd, Edinburgh, 1969).
- ²³ Dorf, E., *Amer. Sci.*, **48**, 341 (1960).
- ²⁴ Colinvaux, P. A., *Ecol. Monogr.*, **34**, 297 (1964).
- ²⁵ Florin, R., *Acta Horti Berg.*, **20**, 121 (1963).
- ²⁶ Wilson, J. T., *Vie et Milieu, sér. B.*, **19**, 395 (1968).
- ²⁷ Dewey, J. F., and Horsfield, B., *Nature*, **225**, 521 (1970).
- ²⁸ Dickinson, W. R., *Science*, **174**, 107 (1971).
- ²⁹ Dewey, J. F., *Sci. Amer.*, **226**, 56 (1972).
- ³⁰ Melville, R., *Nature*, **211**, 116 (1966).
- ³¹ Hallam, A., *Palaeogeogr. Palaeoclimatol. Palaeoecol.*, **3**, 201 (1967).
- ³² Brundin, L., *Ann. Rev. Entomol.*, **12**, 149 (1967).
- ³³ Kurtén, B., *Scientific Amer.*, **220**, 54 (1969).
- ³⁴ Schopf, J. M., *Taxon*, **657** (1970).
- ³⁵ Axelrod, D. I., *Bot. Rev.*, **36**, 277 (1970).
- ³⁶ Middlemiss, F. A., Rawson, P. F., and Newall, G., *Faunal Provinces in Space and Time* (Seel House, Liverpool, 1971).
- ³⁷ Jardine, N., and McKenzie, D., *Nature*, **235**, 20 (1972).
- ³⁸ Fooden, J., *Science*, **175**, 894 (1972).
- ³⁹ Raven, P. H., and Axelrod, D. I., *Science*, **176**, 1379 (1972).
- ⁴⁰ Schuster, R. M., *Bot. Rev.*, **38**, 3 (1972).
- ⁴¹ Hallam, A., *Scientific Amer.*, **227**, 56 (1972).
- ⁴² Wulff, E. V., *An Introduction to Historical Plant Geography* (Chronica Botanica, Waltham, 1943).
- ⁴³ Pitman, W. C., III, and Talwani, M., *Geol. Soc. Amer. Bull.*, **83**, 619 (1972).
- ⁴⁴ Williams, C. A., and McKenzie, D., *Nature*, **232**, 168 (1971).
- ⁴⁵ Laughton, A. S., *Nature*, **232**, 612 (1971).
- ⁴⁶ Phillips, J. D., and Forsyth, O., *Geol. Soc. Amer. Bull.*, **83**, 1579 (1972).
- ⁴⁷ Walter, H., and Straka, H., *Arealkunde* (Ulmer, Stuttgart, 1970).
- ⁴⁸ Manum, S., *Norsk Polarinst. Skr.*, **125**, 1 (1962).
- ⁴⁹ Vogt, P. R., *Nature*, **239**, 79 (1972).
- ⁵⁰ Jäger, E., *Ber. Deut. Botan. Ges.*, **81**, 397 (1968).
- ⁵¹ Hirmer, M., *Handbuch der Paläobotanik, Bd. I* (Oldenburg, München and Berlin, 1927).
- ⁵² Banks, H., et al., in *The Fossil Record* (edit. by Chester, K., and Hughes, N.), 163 (Geological Society, London, 1967).
- ⁵³ Dennis, R. L., *Science*, **163**, 670 (1969).
- ⁵⁴ Dennis, R. L., *Mycologia*, **62**, 578 (1970).
- ⁵⁵ Singer, R., and Archangelsky, S., *Amer. J. Bot.*, **45**, 194 (1958).
- ⁵⁶ Elsik, W. C., *Pollen et Spores*, **10**, 265 (1968).
- ⁵⁷ Rouse, G. E., *Micropaleont.*, **8**, 187 (1962).

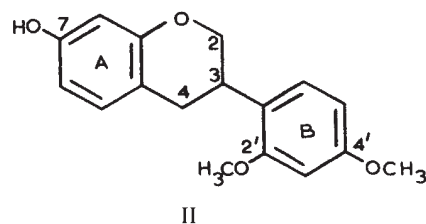
Sativin: an Induced Isoflavan from the Leaves of *Medicago sativa* L.

It has been shown that low-molecular weight antibiotics (phytoalexins¹) may be produced following the interaction between plants and various biotic or abiotic agents. In tissues penetrated by non-pathogenic fungi the accumulation of phytoalexins may be responsible for cessation of fungal development, which suggests that these compounds may have a role in disease resistance. Phytoalexins can apparently be detoxified by pathogenic fungi².

Several phytoalexins have been isolated from species belonging to the Papilionateae subfamily of the Leguminosae^{1,2}. In addition to pisatin from *Pisum sativum*³ and phaseollin from *Phaseolus vulgaris*⁴, investigations of species comprising the tribe Trifoleae have resulted in the isolation of demethylhomopterocarpin (structure I) from *Medicago sativa*⁵, *Trifolium pratense*⁶ and *Melilotus alba* (J. L. Ingham, unpublished observations) and maackiain from *T. pratense*⁶ and *Trigonella foenum-graecum* (J. L. Ingham, unpublished observations). These compounds all belong to the pterocarpin sub-class of the isoflavanoids.



Two antifungal compounds were isolated from infected leaflets of lucerne (*Medicago sativa*). One of these, 3-hydroxy-9-methoxy pterocarpin or demethylhomopterocarpin (structure I) ($\lambda_{\text{max}}^{\text{95\% EtOH}}$ nm, 209, 227(sh), 282 and 287) had been previously isolated from this plant⁵, but the second (for which we propose the name *sativin*) was an isoflavan (structure II) of unknown structure. We report here the detection and characterization of an isoflavan which functions as a phytoalexin.

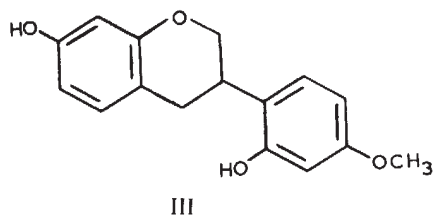


Antifungal materials were obtained from detached leaves by the drop-diffusate technique⁷. Conidial suspensions (about 5×10^4 spores ml⁻¹) of *Helminthosporium turcicum* in 0.05% 'Tween-20' were used as the phytoalexin inducer. Control leaves received drops of de-ionized water containing 'Tween-20'.

After 48 h incubation the droplets (diffusate) were collected and extracted five times with anhydrous ethyl acetate. The ethyl acetate fractions were bulked, reduced to dryness, re-dissolved in 95% ethanol and chromatographed on fluorescent silica-gel thin-layer plates. The developing solvent was a 3:1 (v/v) mixture of chloroform and carbon tetrachloride. In this system sativin had an R_F of 0.44 whilst that of demethylhomopterocarpin was 0.36. Compounds were located on chromatograms either by ultraviolet light or by *p*-nitroaniline reagent⁸. Initially, this reacts with both sativin and demethylhomopterocarpin to produce intense yellow spots, but, whereas the former retains the yellow coloration, the latter rapidly turns a deep orange-brown.

Diffusates were bioassayed directly on thin-layer plates⁹. Irrigated plates were sprayed first with a spore suspension of *Cladosporium cucumerinum* and then with cool, molten 'V-8 juice' agar. After incubation the plates were covered by a grey fungal growth except for those areas corresponding to the location of either sativin or demethylhomopterocarpin. These areas appeared white owing to inhibition of fungal growth. No antifungal activity was detected in control diffusates.

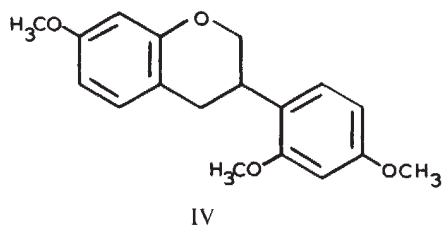
Pure sativin ($C_{17}H_{18}O_4$) is a white crystalline solid possessing a reduced isoflavanoid skeleton substituted with one hydroxyl (OH) and two methoxyl (OCH_3) groups. The mass spectral data (M^+ 286; prominent fragments, m/e 164, 151, 149, 135, 122, 121) indicate an isoflavan ring system. On biogenetic grounds this should be substituted at C-7, C-2' and C-4', and should resemble vestitol (structure III), an isoflavan from *Machaerium vestitum* and *Dalbergia variabilis*¹⁰.



In 95% ethanol sativin has an ultraviolet absorption maxima (nm ($\log_{10} \epsilon$)) at 208 (4.42), 227 (4.01), 280 (3.61), 284 (3.62) and 290sh. (3.42) and minima at 223 (4.00), 251 (2.71) and 283 (3.60). In ethanol containing 0.2 M sodium hydroxide solution, maxima were 216, 248(sh.), 280(sh.), 285 and 296(sh.). Sativin is stable under acid and alkaline conditions and is apparently unaltered by autoclaving at 15 p.s.i. for 30 min. The stability of sativin to refluxing with 4-6% sulphuric acid established that the compound was not a 2'-hydroxyisoflavanone¹¹. In acid media such a compound would undergo cyclodehydration with the consequent formation of a 6a,11a-dehydropterocarpan, recognizable by its characteristic absorbance in the visible (330 to 360 nm) region of the spectrum. Acid stability also indicated the absence of hydroxylation at C-3.

Sativin is soluble in many organic solvents including ethyl acetate, ethanol and methanol. It is sparingly soluble in carbon tetrachloride and virtually insoluble in hot (60° C) or cold (15° C) water. Sativin has a melting point of 125-127° C and is optically active, (α)_D²⁵ -15°. Vestitol has a similar optical rotation (+21.5°) but is dextrorotatory.

Although mass spectral analysis enabled the assignment of the various substituents to either ring-A (hydroxyl) or ring-B (both methoxyl groups) it was considered desirable to prepare synthetically a derivative of sativin, which, when compared with a similar derivative of vestitol, would firmly establish the proposed 7,2',4'-oxygenation pattern. This aim was achieved by methylation of sativin to IV (7,2',4'-tri-



methoxyisoflavan), a compound which can also be prepared by the methylation of vestitol (7,2'-dihydroxy-4'-methoxyisoflavan). The methylated products of sativin and vestitol possessed identical ultraviolet spectra and mass spectral fragmentation patterns (M^+ 300; prominent fragments, m/e 164, 151, 149, 137, 121) and were chromatographically inseparable in several solvent systems. This demonstrated the 7,2',4'-oxygenation pattern of sativin and, together with the mass spectral data, firmly established the substitution pattern of the aromatic rings.

The absolute configuration of sativin is *R*, which follows from its co-occurrence with, and possible derivation from, 6a*R*,11a*R*-(−)-demethylhomopterocarpan. This compound presumably undergoes enzymic cleavage of the dihydrofuran ring (C) followed by selective methylation to sativin. (−)-Vestitol may be an intermediate in this transformation.

When incorporated into agar and tested against the mycelial growth of *H. turcicum*, sativin possessed a median effective dose (ED_{50} value of 15 μ g ml.⁻¹). In contrast, demethylhomopterocarpan had an ED_{50} value of 25 μ g ml.⁻¹. In diffusates the ratio of sativin to demethylhomopterocarpan was approximately 2:1 whilst quantitative analysis of the leaf tissues underlying the droplets consistently revealed a sativan/demethylhomopterocarpan ratio of 5:1. We suggest that sativan has an important role in the disease resistance of lucerne leaflets.

The ring structure of sativin occurs in at least seven other natural products. All known isoflavans except for the animal metabolite, equol¹⁰, have been isolated from the Papilionateae subfamily of the Leguminosae. They include vestitol and mucronulatol from species of *Machaerium* and *Dalbergia*¹⁰, lonchocarpin and laxifloran from *Lonchocarpus laxiflorus* and licoricidin from *Glycyrrhiza glabra*. This is the first description of the isolation of an isoflavan from a species belonging to the tribe Trifoleae. We suggest that because of the marked antifungal activity of sativin and other phytoalexins from the Leguminosae, these, or structurally related synthetic products, may be useful in crop protection.

Full details of this work are in preparation. We thank Professors W. D. Ollis and O. R. Gottlieb for samples of (+)-vestitol.

J. L. INGHAM*
R. L. MILLAR

*Department of Plant Pathology,
Cornell University,
Ithaca, New York 14850*

Received September 5; revised October 12, 1972.

* Present address: Department of Botany, The University, Reading RG6 2AS, England.

¹ Cruickshank, I. A. M., *Ann. Rev. Phytopathol.*, **1**, 351 (1963).

² Ingham, J. L., *Botan. Rev.* (in the press).

³ Perrin, D. R., and Bottomley, W., *J. Amer. Chem. Soc.*, **84**, 1919 (1962).

⁴ Perrin, D. R., *Tetrahedron Lett.*, **1**, 29 (1964).

⁵ Smith, D. G., McInnes, A. G., Higgins, V. J., and Millar, R. L., *Physiol. Plant Pathol.*, **1**, 41 (1971).

⁶ Higgins, V. J., and Smith, D. G., *Phytopathology*, **62**, 235 (1972).

⁷ Higgins, V. J., and Millar, R. L., *Phytopathology*, **58**, 1377 (1968).

⁸ Hathway, D. E., in *Chromatographic and Electrophoretic Techniques*, **1** (edit. by Smith, I.), 308 (Interscience Publishers, NY, 1960).

⁹ Klarman, W. L., and Sanford, J. B., *Life Sciences*, **7**, 1095 (1968).

¹⁰ Ollis, W. D., in *Recent Advances in Phytochemistry*, **1** (edit. by Mabry, T. J., Alston, R. E., and Runeckles, V. C.), 329 (Appleton-Century-Crofts, NY, 1968).

¹¹ Sugimoto, H., *J. Org. Chem.*, **24**, 1655 (1959).

Spittlebug Polymorphic for Warning Coloration

LINDROTH¹ suggests that some insects gain a selective advantage by mimicking other insects whose striking colour patterns serve to warn potential predators of an unusually effective escape mechanism. The mimicked insects hop suddenly when disturbed, greatly lessening the probability of a predator's success. A learning predator comes to associate particular colour patterns with wasted effort and spurns both the model and the relatively sluggish mimic, so long as encounters with the model substantially outnumber those with the mimic. Lindroth's

hypothesis may help explain cases of apparently aposematic (warning) coloration in non-mimetic animals which are neither poisonous, distasteful, nor formidably outfitted for defence, but which possess efficient close quarter escape mechanisms². I propose that the phenomenon of escape warning coloration may be an important factor in the colour polymorphism of *Philaenus spumarius* (L.), the common meadow spittlebug.

This species is one of the most common insects in many North American and Eurasian temperate habitats. It exhibits extensive colour polymorphism, with the number of colour forms and their relative frequencies varying from area to area. Here I limit analysis to the relatively simple polymorphic situation prevailing in the southern Great Lakes region of the United States where only four colour forms (Fig. 1) occur at frequencies exceeding 1%^{3,4}. Breeding experiments in Finland indicate that the colour forms are determined by a series of allelic genes (or supergenes) ordered in a dominance hierarchy⁵. *Populi* and *typica* phenotypes are distinct in males but intergrade in females and represent alternative expressions of one allele. The *trilineata* allele is expressed in both males and females, the *marginella* allele only in females. In heterozygous condition *marginella* is dominant to *populi-typica* (in females) and *trilineata* is always dominant to both these alleles (Halkka, personal communication).

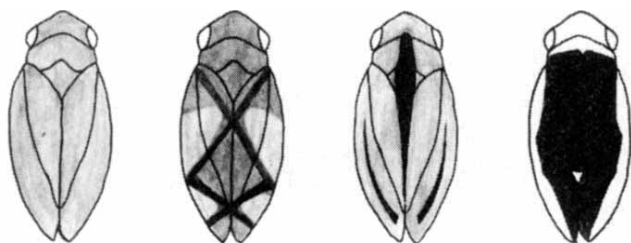


Fig. 1 Main colour forms of *Philaenus spumarius* in southern Great Lakes region. Left to right: *populi*, *typica*, *trilineata*, *marginella*. Actual insect length about 6 mm.

Recent authors⁴⁻⁶ attribute the variety and maintenance of the *P. spumarius* colour polymorphism to apostatic⁷ selection. According to this hypothesis rarer colour forms, even if they are relatively conspicuous, are neglected by predators hunting for prey on the basis of a "search image" formed during their initial contacts with the more common colour forms. Rare forms increase in frequency and common forms decrease until at the frequencies observed in nature all forms have equal probabilities of survival and reproduction. This hypothesis does not explain why particular colour forms have evolved as they have or why their geographical distributions vary in the way that they do. Lindroth's idea provides an approach towards solving these last two puzzles for at least one colour form, *marginella*.

Populi, *typica*, and *trilineata* individuals range in colour from light straw or grey through various shades of nondescript brown with patches or lines of darker brown or black. They appear to be cryptically coloured against a number of natural backgrounds. In contrast, *marginella* individuals with their dark black bodies outlined by cream white on three sides often stand out strikingly from their surroundings. Bold patterns of black and white recur in nature as a theme of warning coloration⁸, and the distinctiveness of the *marginella* colour pattern in *P. spumarius* strongly suggests such a role. The species is not foul tasting (personal observation) or outfitted with obvious aggressive defence mechanisms, and so might seem to lack the ordinary prerequisites for warning coloration. But its back legs, like those of other spittlebugs, are specially modified for hopping. It can fly, but when disturbed it usually jumps abruptly, landing awkwardly several feet away⁹, safely removed from the vicinity of a potential predator.

A learning predator will recall more vividly a frustrating

encounter with *marginella* than a similar encounter with one of the more anonymous colour forms. To the degree that such encounters lead predators to shun *marginella* more than other colour forms, selection will favour *marginella*. If as *marginella* becomes more frequent in a population the selective disadvantage of being common begins to outweigh the advantage of being conspicuous, a balanced polymorphism will result.

The geographical distribution of colour forms and the biology and ecology of *P. spumarius* lend some support to this interpretation. Around the southern Great Lakes the frequencies of most colour forms vary from area to area. *Trilineata*, for example, ranges in frequency from 1% in east central Illinois to 10% in west central Wisconsin. In contrast the relative abundance of *marginella* is remarkably even over the region, comprising about 5% of all females³, in spite of the variety of physical and biological environments the species encounters. But while the species occupies many habitats, its primary habitat throughout the region is fields of leguminous forage crops (principally alfalfa, *Medicago sativa*) where it regularly achieves very high densities (more than 300 adults/m²)¹⁰. Each summer as the fields are mown large numbers of adults disperse over surrounding habitats, swamping local non-pest *P. spumarius* populations. These marginal but substantial populations of woods, roadsides, fencerows, and abandoned land may owe their very existence to the yearly influx from hayfields, a phenomenon thoroughly documented for one old field in south-eastern Michigan¹⁰. This species migration pattern strongly suggests that selective forces operating in fields of forage crops determine *P. spumarius* gene frequencies over the entire region in question. Though the particular forage crops grown and mean climatic factors may vary considerably from area to area within the region, the basic structure of predator-prey relations in the ecology of the fields remains the same, and the frequency of *marginella*, probably determined by these relations, is relatively constant.

The dark black body of *marginella*, in addition to its purely visual role, may assist quick warming in the morning Sun for added mobility in a form that depends for its survival on a reputation for fast getaways. The limitation of *marginella* expression to females probably reflects the fact that, although the species mates soon after adult emergence in June, there is a mechanism for delayed fertilization¹¹ and the females do not begin to oviposit until September^{9,10}. The females carry the species' gametes throughout the summer and must bear the brunt of selection for predator avoidance. In the early summer, probably the only time of selective importance for males, learning predators have had little time to accustom themselves to the pursuit or avoidance of particular colour forms, and the presumed advantages of the *marginella* pattern would be slight or absent. Interestingly, *Neophilaenus lineatus*, a relative of *P. spumarius*, begins laying eggs in early July, soon after adult emergence and mating¹². Although this species is also a widespread pest and reaches high densities in grass meadows in the eastern USA, it is monomorphic.

Conclusive evidence that *P. spumarius* is polymorphic for escape warning coloration will require detailed study of its predator-prey relationships, particularly in alfalfa fields. Birds and spiders are probably major predators¹⁰ and there is good evidence that spittlebugs furnish a significant food source for some sparrow species in south-eastern Michigan (F. C. Evans, unpublished data). Beyond this we know little.

Finally, my arguments apply to a single colour form in a single region. In many other regions the polymorphism is more complex. Even in the region in question it is undoubtedly governed by many more factors than I have included in this analysis.

I thank Drs O. Halkka and F. C. Evans for access to unpublished information, and R. Moscovitch for artistic aid.

VINTON THOMPSON

Committee on Evolutionary Biology,
University of Chicago,
Chicago, Illinois 60637

Received October 20, 1972.

- ¹ Lindroth, C. H., *Entomol. Scand.*, **2**, 41 (1972).
- ² *Nature*, **235**, 312 (1972).
- ³ Thompson, V., and Halkka, O., *Amer. Midland Naturalist* (in the press).
- ⁴ Owen, D. F., and Wiegert, R. G., *Amer. Naturalist*, **96**, 353 (1962).
- ⁵ Owen, D. F., *Nature*, **198**, 201 (1963).
- ⁶ Halkka, O., Raatikainen, M., Halkka, L., and Lallukka, R., *Ann. Zool. Fennici*, **7**, 221 (1970).
- ⁷ Clarke, B., *Systematics Association Publication No. 4*, 47 (1962).
- ⁸ Cott, H. B., *Adaptive Coloration in Animals* (Methuen, London, 1940).
- ⁹ Weaver, C. R., and King, D. R., *Ohio Agr. Expt. Sta. Res. Bull., Wooster*, **741**, 1 (1954).
- ¹⁰ Wiegert, R. G., *Ecol. Monographs*, **34**, 217 (1964).
- ¹¹ Robertson, A., and Gibbs, A. J., *J. Trop. Med. Hyg.*, **40**, 257 (1937).
- ¹² Garman, P., *Conn. Agr. Expt. Sta. Bull.*, **230**, 327 (1921).

Non-Circadian Photoperiodic Timing in the Aphid *Megoura*

The modification of development by the timing of light and darkness, photoperiodism, is well-known among plants, birds, mammals and insects, and its widespread occurrence raises the question of whether it operates by different mechanisms in different instances. The view that all photoperiodism has a common basis is supported by evidence that it often involves an endogenous circadian rhythm (ECR) or "biological clock" that oscillates with similar properties in unrelated organisms^{1,2}. There have been experiments which suggest that one of the systems most carefully studied, the control of sexual and asexual reproduction in the aphid *Megoura viciae*, proceeds through a non-rhythmic, non-oscillating timer of a different kind^{3,4}. Here I report another experiment which provides good evidence against even a rudimentary form of circadian timing in *Megoura* photoperiodism.

The circadian rhythmicity of photoperiodic timing has been inferred from several kinds of evidence; sensitivity to brief light exposures during long dark periods may show maxima or minima roughly 24 h apart, and light/dark cycles with overall periodicities close to 24 or 48 h may have unique effects. A more complex type of experiment involves so-called "skeleton photoperiods". Instead of a normal daily cycle such as 8 h of light alternating with 16 h of darkness (here abbreviated as 8(16)) a schedule such as 1(6)1(16) can be used, in which the 8 h light period is represented by its "skeleton", a dark period started and terminated by brief light exposures including a total span of 8 h. Circadian rhythms in quite diverse phenomena, such as pupal eclosion in the fly *Drosophila* and carbon dioxide output by cultures of the duckweed *Lemna*, respond similarly to skeleton photoperiods in several ways^{2,5,6}. In this context it is important that when the two portions of the skeleton photoperiod are nearly equal, for example in 1/4(10½)1/4(13), the effect on the oscillation of the rhythm depends on which of the two dark periods is presented first (after continuous light).

This result prompted the use of skeleton photoperiods on *Megoura* to test the hypothesis that photoperiodic timing in this aphid is effected by an endogenous circadian rhythm with properties similar to those of *Drosophila* and *Lemna*. The opposing hypothesis, based on earlier work with *Megoura*, is that there is a non-rhythmic timer that simply measures each dark period. Similar skeleton photoperiod experiments have been reported with *Pectinophora* but the light periods used were apparently too short to affect the photoperiodic response at all⁷.

Lees's clone of *Megoura viciae* Buckton was used, with frequent transfer to fresh seedlings of a dwarf *Vicia faba* to avoid crowding and thus ensure that all females were apterae (wingless). Stock cultures were raised at approximately 15° C under continuous cool, white fluorescent light

of about 1,000 L, a condition in which all females produced are virginoparae, producing only embryos rather than ova. The same temperature and light intensity were used for the experimental treatments.

I began by transferring several fourth instar apterae from the stock conditions to those being tested, where they produced abundantly the offspring which were the actual individuals under treatment. The original apterae were generally discarded after 7 days, and after a further 7 days, 21 individuals from each treatment were removed to ordinary room conditions and segregated in such a way that their subsequent offspring could be collected and identified. Approximately two weeks later, the families of offspring produced by each individual were preserved in 25% ethanol containing 1 g⁻¹ phenylthiourea. Ten of the largest (oldest) in each family were later examined to determine whether they were virginoparae or oviparae.

Results are presented as the percentage (VP%) of treated individuals from a total of 18 to 21, depending upon mortality, whose ten largest offspring include five or more virginoparae. In preliminary experiments, VP% was 100 when the 14-day treatment was entirely under continuous light; VP% was 0 when the entire 14 days was under a schedule of 11(13), (11 h light, 13 h darkness) and VP% was 40 with 14 1/4 (9 3/4), and also with first 7 days under 11(13) and the next 7 under continuous light. These results agree closely with those previously reported^{3,4,8}.

Results and the relative values expected within each pair or group of treatments on the ECR hypothesis are summarized in Table 1. These expectations were never fulfilled.

For experiments A to D to support the ECR hypothesis and conform to results with *Lemna* and *Drosophila*, the expectations were that the schedule 1(9)1(13), started immediately following continuous light, should have a long-day effect, as if the 9-h dark period were being read as the night phase; 1(13)1(9), under the same circumstances, should have a short-day effect, with the 13-h dark period read as night phase; and an initial 12 h of darkness should convert each such result to its opposite by displacing the phase relationship by half a circadian cycle^{2,6}. Only saturatingly high long-day effects, however, were obtained in all four treatments.

There are two possible objections to these experiments. First, the *Lemna* work suggests that the differences sought might well be unstable, occurring during the first few days but easily obscured in a two week treatment^{6,9}. In *Lemna*, such instability results in the short-day and not the long-day effect, but the reverse might occur in *Megoura*. Second, even stable but small differences in the potential action of treatments could be obscured by continuing the exposure to saturation. I therefore undertook experiments E to G in which two of the schedules used previously were given as 7-day pretreatments to an unambiguously short-day schedule 12(12), in order to produce intermediate VP% values. Both pretreatments also had approximately the same effect which disagrees with the ECR hypothesis. Even if the small difference should prove statistically significant or survive a longer series of experiments, it would be in the opposite direction. Such a result would precisely accord with the non-circadian hypothesis, as there is one more short (9 h) night in the schedule giving the higher values⁴.

The experiments A to G depend upon the assumption that the two dark periods in 1(9)1(13) and 1(13)1(9) are sufficiently similar so that for a circadian system, the schedules are ambiguous and effects thus depend on the initial dark period. The decisively long-day effects obtained, however, made it necessary to modify the schedules to increase the probability of a short-day response. Changing a previously long-day schedule should at some point result in ambiguity if the ECR hypothesis is correct. On this hypothesis, further reducing the length of the shorter dark period should increase the likelihood of the longer one being read

Table 1 Photoperiodic Response of *Megoura viciae* to Skeleton Photoperiods, and Expectations on the Endogenous Circadian Rhythm (ECR) Hypothesis

Experiments	Light (dark) schedule (h) Day 1	Days 2-7	Days 8-14	Virginopara-producers; percentages and means	ECR expectations
{ A-C A-C	1(9)1(13) 12(12)	1(9)1(13)	→	100, 100, 100: 100, 100, 100: 100	High Low
{ A, B, D, E A, B, D, E	1(13)1(9) 12(12)	1(13)1(9)	→	100, 100, 100, 100: 100, 100, 100, 100: 100	Low High
{ E-G E-G	1(13)1(9) 12(12)	1(13)1(9)	12(12)	76, 43, 55: 62, 38, 40: 58 47	Low High
{ H-J H-J	4(3)3(13) 11(13)	4(3)4(13)	→	0, 0, 0: 0, 0, 0: 0	High Low
{ H, I, K, M-O H, I, K	3(5)3(13) 11(13)	3(5)3(13)	→	10, 30, 20, 30, 5, 20: 25, 15, 15: 19 18	High Low
M-O	3(13)3(5)	→	→	15, 17, 15: 16	Low
{ L-O L-O	2(7)2(13) 2(13)2(7)	→ →	→ →	90, 90, 70, 95: 90, 95, 75, 90: 8y 88	High Low

as the night phase, and so schedules 4(3)4(13), 3(5)3(13) and 2(7)2(13) were tested. These schedules should also have an increased short-day tendency on the non-circadian mechanism because light-interruptions are minimally effective in *Megoura* after about 4 h of darkness, and earlier interruptions only reset the dark period timer to its starting condition^{3,4}. Increased short-day tendency (resulting in ambiguity) should, under the circadian hypothesis, increase the susceptibility of the effect to inversion by an initial 12- or 13-h dark period. The non-circadian hypothesis does not require this prediction. Effects expected on the basis of the ECR hypothesis were not to be found (Table 1).

Lees concluded that *Megoura* photoperiodism does not involve an endogenous circadian rhythm on the basis of experiments with long dark periods and with light/dark cycles of various lengths^{3,4}. The first type of experiment depends on the assumption that a circadian oscillation would persist through the dark periods, while the second assumes that such a timer would be severely limited in its capacity to entrain to non-circadian periodicities. There are some rhythms, as for example^{5,6} in *Lemna*, that persists only very weakly without external signals, and even the highly persistent rhythm of carbon dioxide output in *Bryophyllum* cannot be entrained to a 6-h periodicity¹⁰. Neither persistence beyond one cycle nor resistance to non-circadian periodicities, whatever their value to other processes, has any obvious role in a photoperiodic timer, and it would not be surprising to find one in which both characteristics had been lost. If such a timer had originated in a more typical ECR system, and still retained some of its basic features, these features might be revealed under skeleton photoperiods as these supply repeated 24-h signals while examining the response to other inputs.

None of the effects of skeleton photoperiods on *Megoura* resemble those with *Lemna* or *Drosophila* and all can be accounted for by the non-circadian scheme proposed by Lees^{3,4}, this is overwhelming evidence against even a rudimentary form of circadian timing in *Megoura* photoperiodism. The attractive concept of one universal timing mechanism in photoperiodism seems thus even more difficult to maintain.

I am grateful for a National Science Foundation Senior Postdoctoral Fellowship and a John Simon Guggenheim

Memorial Fellowship; I thank Professor A. D. Lees and the Imperial College of Science and Technology for hospitality and Mr M. Nicholls for technical assistance.

W. S. HILLMAN*

Photoperiod Laboratory, ARC Insect Physiology Group,
Department of Zoology and Applied Entomology,
Imperial College Field Station, Ascot, Berkshire

Received September 21, 1972.

* Present address: Biology Department, Brookhaven National Laboratory, Upton, NY 11973.

¹ Bünning, E., *Photochem. Photobiol.*, **9**, 219 (1969).

² Pittendrigh, C. S., *Z. Pflanzenphysiol.*, **54**, 275 (1966).

³ Lees, A. D., *Nature*, **210**, 986 (1966).

⁴ Lees, A. D., in *Proc. Int. Symp. Circadian Rhythmicity*, 87 (Centre for Agricultural Publishing and Documentation, Holland, 1972).

⁵ Hillman, W. S., *Plant Physiol.*, **45**, 273 (1970).

⁶ Hillman, W. S., *Amer. Naturalist*, **98**, 323 (1964).

⁷ Pittendrigh, C. S., and Minis, D. H., in *Biochronometry* (edit. by Menaker, M.), 212 (National Academy of Science, Washington, 1971).

⁸ Lees, A. D., *J. Insect Physiol.*, **13**, 1781 (1967).

⁹ Hillman, W. S., *Plant Physiol.*, **49**, 907 (1972).

¹⁰ Wilkins, M. B., in *The Physiology of Plant Growth and Development* (edit. by Wilkins, M. B.), 647 (McGraw-Hill, London, 1969).

Batesian Mimicry between *Danaus chrysippus* and *Hypolimnys misippus* (Lepidoptera) in Tanzania

Danaus chrysippus (Danaiidae) is a common butterfly of open country throughout tropical Africa¹. The species is reputed to be distasteful and there is experimental evidence of this for a related American species, *D. plexippus*, in which the associated warning coloration has also been shown to be effective against predators^{2,3}. *D. chrysippus*, which also shows warning coloration, is strikingly polymorphic over much of its range and four sympatric forms have been described from Uganda¹. It acts as



Fig. 1 The four colour forms of *Danaus chrysippus* (left) and the corresponding female forms of *Hypolimnas misippus*. The forms of *D. chrysippus*, in order from the top, are named *chrysippus*, *alcippus*, *dorippus*, and *albinus*. The corresponding forms of *H. misippus* are *misippus*, *alcippoides*, *inaria* and *inaria-alcippoides*. The black and white areas in the figure are black and white as shown and the shaded area is orange. The male of *H. misippus* is black and white and quite different from any of the female forms (from ref. 6).

a model for several mimetic butterflies and is commonly involved in a supposed Batesian complex with *Hypolimnas misippus* (Nymphalidae) of which there are also four distinct female morphs⁴⁻⁶, each resembling closely a morph of *D. chrysippus*. (Male *H. misippus* are monomorphic, black and white and not mimetic.) There has been some doubt, however, about the efficiency of the mimicry as some morphs of *H. misippus* may be common in the absence of their *D. chrysippus* models. Also the abundance of the edible mimic is sometimes greatly in excess of that of the model⁶, which does not conform to what would normally be expected from Batesian mimicry theory^{6,7}.

My study is based on regular sampling for both *D. chrysippus* and *H. misippus* in rough grassland on the campus of the University of Dar es Salaam, Tanzania. All four described morphs of both species (Fig. 1) are present, though some are rare. Intermediate forms of *H. misippus* with respect to both

forewing and hindwing coloration occur and no individual with a fully developed white hindwing patch was seen. Six forms are therefore scored; all *alcippoides* and *inaria-alcippoides* were animals with some white on the hindwing but not the full white patch. The results of five months' sampling are shown in Table 1.

Table 1 Distribution of Female Morphs in *Hypolimnas misippus**

Hindwing	Morph	Number examined	% frequency of ♀ morphs
Plain orange	<i>misippus</i>	15	34.1
	transitional forewing	6	13.6
	<i>inaria</i>	16	36.4
Orange with white patch	<i>alcippoides</i>	3	6.8
	transitional forewing	3	6.8
	<i>inaria-alcippoides</i>	1	2.3
Total		44	100.0

* Collected between February and June 1972 at Dar es Salaam.

The frequency of the four morphs of *D. chrysippus* over the same period is given in Table 2. The forms scored as *alcippus* and *albinus* did not have a fully developed white patch and may be heterozygotes¹.

D. chrysippus (309 caught) was much more abundant than *H. misippus* (99 caught, 44 female and 55 male) as expected⁶. If the palatable mimic approaches or exceeds its model in abundance, predators will probably quickly learn to recognize a phaneric pattern associated with palatability and the safety of both mimic and model is threatened. The relationship gives selective advantage only when the mimic is relatively rare. There is experimental evidence^{2,8} that birds will eat warningly coloured edible mimics if they have not had previous experience of the distasteful model.

Table 2 Distribution of Morphs in *Danaus chrysippus**

Morph	Number examined	% frequency
<i>chrysippus</i>	132	42.7
<i>dorippus</i>	171	55.3
<i>alcippus</i>	4	1.3
<i>albinus</i>	2	0.7
Total	309	100.0

* Collected between February and June 1972 at Dar es Salaam.

Because *H. misippus* is a fast flying butterfly and not as easy to catch as *D. chrysippus*, the comparative numbers of the two species caught may not be indicative of their true proportions. Female *H. misippus*, however, amount to 14.3% of the *D. chrysippus* (both sexes) caught and this figure is close to other estimates of the ratio of mimic to model^{6,8} in tropical mimetic assemblages.

This investigation is the first where all four forms of both species are sympatric. The figures given in Tables 1 and 2 show that the frequencies of the four forms of the mimic are similarly ranked to the corresponding forms of the model (the forms of *H. misippus* with a transitional forewing are omitted from the ranking). The probability of such a ranking is 1/24 and is therefore statistically significant at the 5% level. This result suggests that mimetic resemblance is an important factor influencing natural selection in *H. misippus*. Heterosis has been postulated to explain the existence of forms of the mimic where the corresponding model is absent⁷, as in Ghana and India, but is unnecessary for the populations described here.

While mimetic resemblance is sufficient to explain the polymorphism in *H. misippus*, it does not account for that of the model. *D. chrysippus* is involved in a Müllerian mimetic assemblage with another distasteful butterfly, *Acraea encedon*

(Acraeidae) in the region of Dar es Salaam as in other parts of Africa⁹, and is also mimicked by other species. It has been suggested¹ that the polymorphism of *D. chrysippus* is a means of breaking down to some extent the resemblance of mimics to the model, as the model will have a selective disadvantage if it has too many mimics.

My results are in agreement with the classical mimicry theory that the mimic is much less abundant than its model and the frequencies of the four morphs are identically ranked in the two species.

Further results will be published elsewhere.

D. A. S. SMITH

Department of Zoology,
University of Dar es Salaam,
PO Box 35064,
Dar es Salaam

Received August 31, 1972.

¹ Owen, D. F., and Chanter, D. O., *Rev. Zool. Bot. Afr.*, **78**, 81 (1968).

² Brower, J. V. Z., *Evolution*, **12**, 32 (1958).

³ Brower, J. V. Z., and Brower, L. P., *Zoologica*, **49**, 137 (1964).

⁴ Edmunds, M., *Nature*, **212**, 1478 (1966).

⁵ Edmunds, M., *Heredity*, **24**, 281 (1969).

⁶ Owen, D. F., *Tropical Butterflies* (Clarendon Press, Oxford, 1971).

⁷ Ford, E. B., *Ecological Genetics* (Methuen, London, 1964).

⁸ Carpenter, G. D. H., *Trans. Roy. Ent. Soc. Lond.*, **100**, 71 (1949).

⁹ Owen, D. F., *Animal Ecology in Tropical Africa* (Oliver and Boyd, Edinburgh, 1966).

Negative Non-random Mating in the Polymorphic Butterfly *Danaus chrysippus* in Tanzania

FOUR distinct morphs of the common savannah butterfly *Danaus chrysippus* are found in the region of Dar es Salaam, Tanzania. These forms have been described¹ and their relative frequencies on the campus of the University of Dar es Salaam during 5 months of sampling are known². The *dorippus* and *chrysippus* forms are relatively common while *alcippus* and *albinus* are both rare.

Table 1 Sex Ratios in a Population of *Danaus chrysippus* between February and July 1972

Month	Number examined	<i>dorippus</i>			<i>chrysippus</i>		
		%♀	%♂	χ^2	%♀	%♂	χ^2
Feb.	26	68.4	31.6	2.58	42.9	57.1	—*
Mar.	28	47.4	52.6	0.05	33.3	66.7	—*
Apr.	51	56.2	43.8	0.50	57.9	42.1	0.47
May	81	27.3	72.7	9.09†	45.9	54.1	0.24
June	117	26.3	73.7	12.79‡	40.0	60.0	2.40
July	123	26.5	73.5	15.06†	43.6	56.4	1.47

* Sample too small to calculate χ^2 . † $P < 0.01$, ‡ $P < 0.001$.

To calculate the frequencies of pairings expected as a result of random mating it has not been possible to assume a 1:1 sex ratio throughout the sampling period. The results indicate that the sex ratio, particularly in form *dorippus*, is far from constant. Monthly sampling figures from February to July 1972 are given in Table 1. Highly significant departures from a 1:1 sex ratio are apparent in the *dorippus* form from May to July when there is an excess of males over females. None of the values of χ^2 for the sex ratio in *chrysippus* shows a significant departure from a 1:1 expectation. It has been pointed out¹ that the sex ratios of samples taken in the field may not be an accurate reflexion of the true sex ratio because males are more

active than females and therefore more liable to be caught. The type of activity also influences the results, males being commoner in samples taken while feeding at flowers and females predominating during egg laying activity. The lack of departure, however, from the expected 1:1 ratio in form *chrysippus* throughout and in form *dorippus* from February to April, suggests that the departure from a 1:1 ratio in *dorippus* from May to July is a real one. An alternative possibility is a change in the behaviour of *dorippus* males which renders them more liable to capture, but this is unlikely since the ratio of *dorippus* males to *chrysippus* males over the whole period has been constant ($\chi^2_{(5)} = 1.49$; $0.95 > P > 0.90$), while the corresponding ratio for females changed greatly over the same period ($\chi^2_{(5)} = 14.65$; $0.02 > P > 0.01$) (Table 2). Consequently, expected values for the frequencies of pairings assuming random mating, have been made on a monthly basis using the observed sex ratios in each sample.

Records of mating frequencies in the field are available for all 6 months during which 31 mating pairs have been seen. Results are given for the *dorippus* and *chrysippus* forms only, as the rare *alcippus* and *albinus* forms were not seen mating. The observed and expected frequencies of pairings assuming random mating are given in Table 3. The value of χ^2 based on the four possible matings is statistically significant at the 5% level and a comparison of like with unlike matings gives a more significant value ($\chi^2_{(1)} = 6.33$; $0.02 > P > 0.01$).

Table 2 Morph Ratios in a Population of *Danaus chrysippus** between February and July 1972

Month	Number examined	Males		Females	
		<i>dorippus</i>	<i>chrysippus</i>	<i>dorippus</i>	<i>chrysippus</i>
Feb.	26	60.0	40.0	81.3	18.7
Mar.	28	62.5	37.5	75.0	25.0
Apr.	51	63.6	36.4	62.1	37.9
May	81	61.5	38.5	41.4	58.6
June	117	53.8	46.2	38.5	61.5
July	123	61.7	38.3	42.9	57.1

* Small numbers of the morphs *alcippus* and *albinus* are omitted from the table.

Table 3 Frequencies of Observed Matings in a Population of *Danaus chrysippus*

Month	<i>dorippus</i> ♂ % <i>dorippus</i> ♀		<i>dorippus</i> ♀ % <i>chrysippus</i> ♀		<i>chrysippus</i> ♂ % <i>dorippus</i> ♀		<i>chrysippus</i> ♀ % <i>chrysippus</i> ♀	
	♂	♀	♂	♀	♂	♀	♂	♀
Feb.	2 (1.96)	0 (0.45)	2 (1.30)	0 (0.30)	0 (0.47)	0 (0.47)	0 (0.47)	0 (0.47)
Mar.	2 (1.41)	1 (0.28)	0 (0.84)	0 (0.84)	0 (0.47)	0 (0.47)	0 (0.47)	0 (0.47)
Apr.	0 (1.58)	2 (0.96)	2 (0.90)	0 (0.55)	0 (0.47)	0 (0.47)	0 (0.47)	0 (0.47)
May	1 (1.53)	1 (2.16)	3 (0.95)	1 (1.36)	1 (1.36)	1 (1.36)	1 (1.36)	1 (1.36)
June	0 (1.45)	3 (2.32)	2 (1.25)	2 (1.99)	2 (1.99)	2 (1.99)	2 (1.99)	2 (1.99)
July	1 (1.86)	3 (2.46)	3 (1.15)	0 (1.53)	0 (1.53)	0 (1.53)	0 (1.53)	0 (1.53)
Total	6 (9.79)	10 (8.63)	12 (6.39)	3 (6.20)	3 (6.20)	3 (6.20)	3 (6.20)	3 (6.20)
χ^2	1.467	0.217	4.925	1.652	1.652	1.652	1.652	1.652

$\Sigma \chi^2_{(3)} = 8.261$; $0.05 > P > 0.02$.

Expected frequencies assuming random mate selection in parentheses.

Observations so far suggest that two mechanisms exist in this population which would be expected to perpetuate a polymorphism. First, negative non-random mating occurs in which the *chrysippus* form is more likely to mate with *dorippus* than with its kind. This is particularly so for matings between *chrysippus* males and *dorippus* females which are much more numerous than expected. This effect is not the result of differential success in mating by either sex of either morph. Both sexes and morphs are involved in mating pairs in the proportions expected from the composition of the population ($\chi^2_{(1)}$ for males = 0.329; $0.80 > P > 0.50$; $\chi^2_{(1)}$ for females = 0.147; $0.80 > P > 0.50$). Second, the excess of the *dorippus* form among males and *chrysippus* among females between May and July (Table 2),

which has been allowed for in calculating random expectations, must contribute independently to the maintenance of the polymorphism, because each is more likely to encounter the other morph in searching for a mate. The mechanism responsible for the unequal sex ratio in *dorippus* is not yet clear, but it seems that the phenomenon may be seasonal. It is hoped that further observations will clarify this.

Non-random mating has not been reported often in Lepidoptera although it has long been known in the arctiid moth, *Panaxia dominula*³ where it is responsible for the maintenance of the *medionigra* gene in the population.

D. A. S. SMITH

Department of Zoology,
University of Dar es Salaam,
PO Box 35064,
Dar es Salaam

Received August 31, 1972.

¹ Owen, D. F., and Chanter, D. O., *Rev. Zool. Bot. Afr.*, **78**, 81 (1968).

² Smith, D. A. S., *Nature*, **242**, 129 (1973).

³ Sheppard, P. M., *Heredity*, **6**, 239 (1952).

Marine and Freshwater Skaters: Differences in Surface Fine Structures

TRACHAETE arthropods, notably insects, are almost totally absent from the open ocean for various reasons which are still largely speculative¹⁻³. *Halobates* (Heteroptera) is an exceptional insect genus which is exclusively marine, with several open-ocean species that spend their entire lives thousands of miles away from land. It is a member of the family Gerridae, which includes the common pond-skaters or water-striders. Although species of *Halobates* have been known for more than 150 yr, there is little information about their biology and about specific adaptations which might enable them to live in the oceans where all other insects have failed⁴.

Some of the special adaptive features of *Halobates* that may be involved in the conquest of their unusual habitat probably lie in the body surface, and we examined surface fine structures of *Halobates germanus* White, from the central Pacific Ocean, and *H. proavus* White, from the Malayan coast. A related

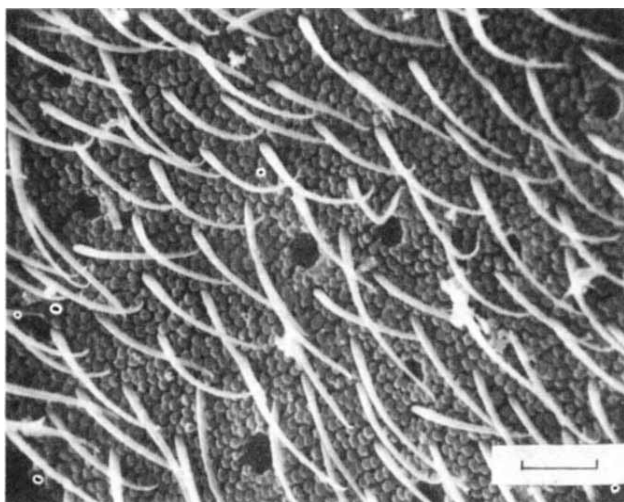


Fig. 1 *Halobates proavus*, dorso-lateral region of mesothorax showing hairs, pits, and mushroom-like microtrichia. (Scale=10 μ m.)

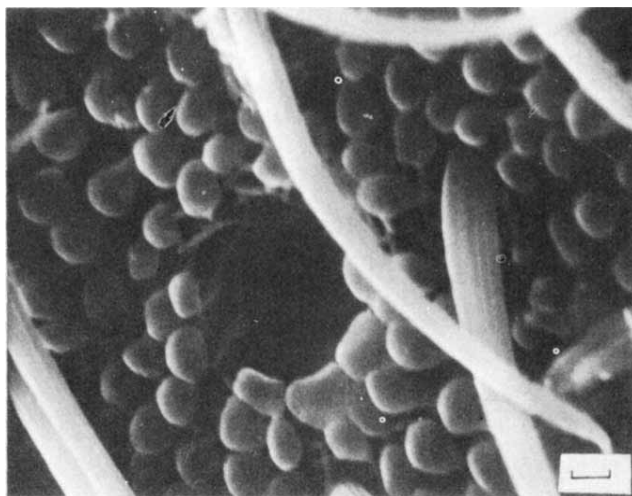


Fig. 2 *H. proavus*, same as Fig. 1. (Scale=1 μ m.)

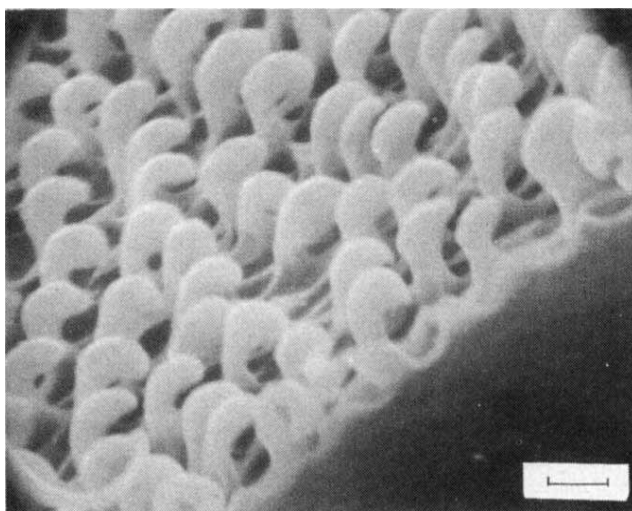


Fig. 3 *H. germanus*, lateral view of section of mesothorax showing microtrichia. (Scale=1 μ m.)

freshwater *Ventidius* species of the same subfamily (Halobatinae) was also studied for comparison.

All the scanning electron micrographs shown here are of dorsal thoracic surfaces. Figs. 1 to 3 are of *Halobates* (surface fine structures of the two species studied were similar) and Figs. 4 and 5 are of *Ventidius*. In both genera, "pits" or "depressions" are found on the head and thorax, being more abundant on the sides than in the middle. Surface fine structures of the dorsal abdominal region are very similar to those of the thorax, although there are fewer pits; none have been found on the ventral surface.

The most striking difference between the two genera is in the shape of the microtrichia forming, presumably, the plastron, a thin film of gas supported by a layer of fine hairs which are set sufficiently close together to be resistant to wetting. The plastron serves to provide and maintain enough oxygen to satisfy the respiratory requirement of a submerged insect⁵. In *Ventidius* the microtrichia are simple pegs (Fig. 5) about 1 μ m high, 0.3 μ m wide at the base, and spaced 0.5 μ m apart. In *Halobates* they appear from the top view like mushrooms, but from the side they are revealed to be thick hooks with the tips bent at about 90° (Fig. 3). These hook-like structures are about 1.5 μ m high, 1.0 μ m wide across the top, 0.5 μ m wide at the base, and spaced 1.5 μ m apart, the average "inter-

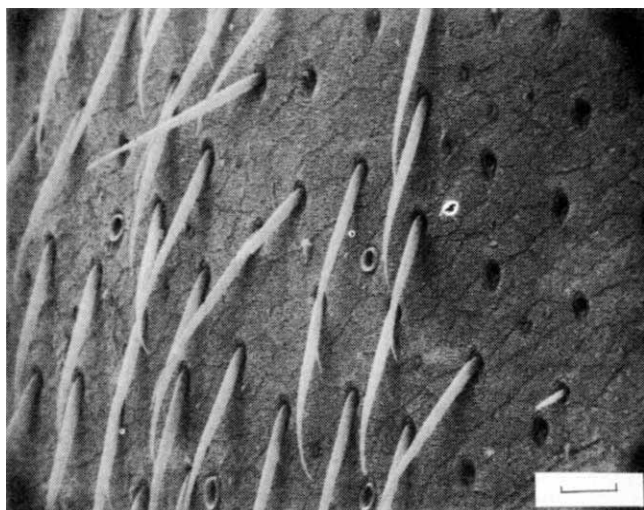


Fig. 4 *Ventidius* species; dorso-lateral region of mesothorax showing hairs, hair base, pits, and velvety mat of microtrichia. (Scale = 10 μ m.)

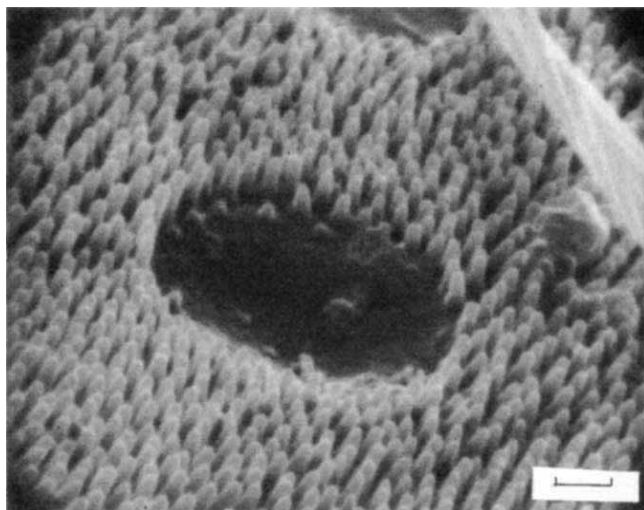


Fig. 5 *Ventidius* sp. Same as in Fig. 4, showing pit and peg-like microtrichia. (Scale = 1 μ m.)

cap" distance being 0.5 μ m. They are quite unlike the plastron of certain intertidal flies (Diptera) studied by Hinton^{6,7}. According to Thorpe and Crisp⁸, such an arrangement of bent hairs, as seen in *Halobates*, can be expected to form a more efficient plastron than a velvety layer of straight hairs.

The evident differences in surface fine structures between marine and freshwater skaters may be expected to have some biological or ecological significance. Species of *Ventidius*, which normally skate on the surface of running water in tropical streams, presumably sometimes need to dive (as has been observed for *Gerris* sp.) to lay eggs on submerged objects and would therefore be expected to have developed less water repellent surfaces, which would permit their occasional submergence. On the other hand, *Halobates* sp., being restricted to the ocean surface, without protection from rain, spray or waves, would presumably require their surfaces to be as water-repellent as possible in order to minimize the danger of drowning during periods of accidental submergence. They might therefore be expected to have developed a highly efficient plastron, as well as a markedly hydrofuge body covering. These features

would not be expected to handicap them during oviposition, as they lay their eggs exclusively on flotsam⁴.

L. CHENG

*Scripps Institution of Oceanography,
La Jolla, California 92037*

Received November 13, 1972.

- ¹ Pruthi, H. S., *Nature*, **130**, 312 (1932).
- ² Thorpe, W. E., *Nature*, **130**, 629 (1932).
- ³ Usinger, R. L., *Geol. Soc. Amer. Mem.*, **67**, 1177 (1957).
- ⁴ Cheng, L., *Oceanography and Marine Biology: An Annual Review* (in the press).
- ⁵ Thorpe, W. H., *Biol. Rev.*, **25**, 344 (1950).
- ⁶ Hinton, H. E., *Nature*, **209**, 220 (1966).
- ⁷ Hinton, H. E., *J. UK Mar. Biol. Ass.*, **47**, 319 (1967).
- ⁸ Thorpe, W. H., and Crisp, D. J., *J. Exp. Biol.*, **24**, 227 (1947).

Acceleration by Ecdysterone of Premolt Substages in the Crayfish

AN insect does not enter premolt if its thoracic glands have been removed, nor a crustacean if its y-organs have been removed, but once premolt is under way it continues whether or not the glands are present^{1,2}. This suggests that the hormone secreted by these glands acts merely as a trigger to initiate premolt, and as the injection of ecdysone also initiates premolt, this is the hormone thought to be secreted. Also, the brain-ring gland complex of *Calliphora*, which contains the homologues of the thoracic glands, has been shown to produce substances similar to ecdysone and crustecdysone³.

If ecdysone acts only as a trigger, then injections of ecdysone should have no effect on any stages of premolt development except the initial stage. Consistent with the trigger hypothesis, ecdysone produces an effect on insect pupal development only if injected early enough⁴. Ecdysone, however, is known to affect many diverse processes most of which are not restricted to early premolt, such as dopa decarboxylase synthesis⁵, conversion of glucose to trehalose⁶, cell movement^{7,8}, calcification⁹, apolysis^{10,11}, formation of gastroliths¹², and many more. Levels of ecdysone rise and fall during the moult cycle¹³⁻¹⁷, which also suggests that the hormone has separate control functions over different processes during the cycle. Furthermore, the effects of ecdysone vary according to the amount present; for example, Rao (personal communication) found that the injection of 5 μ g of ecdysterone per animal into fiddler crabs caused apolysis but not moulting. Others found that the time required to initiate premolt after ecdysterone injection into crayfish varied inversely with the dose¹¹, while injected ecdysterone speeded the moult of animals already in premolt at the injection time¹⁸, and crayfish induced to enter premolt by eyestalk removal moulted sooner if injected with ecdysterone^{12,19}. Ohtake, Milkman and Williams²⁰ found evidence for a covert accumulation of the effect of ecdysone on the tissues. Thus, ecdysone does not produce a single event, but an accumulative series of events as hormone secretion continues over a span of time.

Ecdysone stimulates the production of additional ecdysone¹⁵, a positive feedback system, which may explain how ecdysone can serve both as a trigger and controller. Thus, an injection of ecdysone may stimulate the production of sufficient hormone to produce all the changes necessary for moulting. The thoracic glands or y-organs need not be present for this response because other organs are capable of ecdysone synthesis, as shown by the fact that isolated abdomens of the silkworm *Bombyx* convert labelled cholesterol into ecdysone²¹.

Little is known about the effects of ecdysone in crustaceans. How early in premolt must ecdysone be injected in order to have an effect? Does ecdysone stimulate events in every stage

of premoult or only in some? Can the progression from each premoult stage to the next be speeded by ecdysone? Our investigation was designed to answer these questions.

Specimens of *Orconectes sanborni* were collected in a stream near Kent, Ohio, and maintained in the laboratory, each in a separate, partitioned cubicle of an acrylic plastic aquarium containing tap water recirculating through calcareous gravel. Each experimental animal was matched with a control animal of the same sex, weight, and moulting stage; only crayfish in intermoult and premoult stages were used. Experimentals were injected with $2 \mu\text{g g}^{-1}$ body weight of ecdysterone (Mann Research Laboratories) dissolved in Van Harreveld²² solution at a concentration of $2 \mu\text{g}$ per $10 \mu\text{l}$, and controls were injected with the same quantity of Van Harreveld solution alone. After injection, the moulting stage of each animal was determined as described by Stevenson²³⁻²⁵ at 24 h intervals for at least seven days. Animals were randomly assigned to compartments and identified by code so that the group and past history of an animal would not be known at the time of staging.

Table 1 Injection of Ecdysterone into Crayfish at Various Moulting Stages

Initial moulting stage	Mean difference, in days	<i>p</i> less than:	<i>n</i> *	<i>s_x</i> †	<i>t</i> ‡
C ₃₋₄	2.56	0.0005	9	0.34	7.53
D ₀	6.9	0.0005	10	0.71	9.71
D ₁ '	2.5	0.0005	10	0.43	5.81
D ₁ "	1.17	0.05	6	0.48	2.44
D ₂	1.5	0.005	8	0.42	3.57

* Number of pairs in each group.

† Standard error of difference.

‡ Student's *t* test.

In the case of every moulting stage studied, the crayfish injected with ecdysterone entered the next succeeding moulting stage significantly sooner than those injected with the Van Harreveld solution alone. Table 1 shows the moulting stage of each group at the time of injection, the mean difference between the number of days required by each experimental animal to enter the next succeeding moulting stage and the number of days required by the corresponding matched control animal, and the probability of such a difference occurring by chance according to a one-tailed Student's *t* test. The table also shows the number of pairs in each group (*n*), the standard error of the difference *s_x* and *t*. Every value of *P* was below 0.05, and most were much lower.

These results show that ecdysterone not only causes the initiation of premoult, but continues, during the premoult stages studied, to stimulate the various processes that prepare the animal for the next moult.

J. R. STEVENSON
J. A. TSCHANTZ

Department of Biological Sciences,
Kent State University,
Kent, Ohio 44242

Received October 18, 1972.

¹ Echallier, G., *CR Acad. Sci.*, **238**, 523 (1954).

² Wigglesworth, V. B., *The Physiology of Insect Metamorphosis* (Cambridge Univ. Press, 1954).

³ Willig, A., Rees, H. H., and Goodwin, T. W., *J. Insect Physiol.*, **17**, 2317 (1971).

⁴ Williams, C. M., *Biol. Bull.*, **134**, 344 (1968).

⁵ Karlson, P., and Sekeris, C. E., *Biochim. Biophys. Acta*, **63**, 489 (1962).

⁶ Kobayashi, M., and Kimura, S., *J. Insect Physiol.*, **13**, 545 (1967).

⁷ Judy, K. J., *Science*, **165**, 1374 (1969).

⁸ Reinecke, J. P., and Robbins, J. D., *Exp. Cell Res.*, **64**, 335 (1971).

⁹ Fraenkel, G., and Hsiao, C., *J. Insect Physiol.*, **13**, 1387 (1967).

¹⁰ Krishnakumaran, A., and Schneiderman, H. A., *Biol. Bull.*, **139**, 520 (1970).

¹¹ Warner, A. C., and Stevenson, J. R., *Gen. Comp. Endocrinol.*, **18**, 454 (1972).

¹² Lowe, M. E., and Horn, D. H. S., in *Physiological Systems in Semiarid Environments* (edit. by Hoffman, C. C.), 155 (Univ. New Mexico Press, Albuquerque, 1969).

¹³ Shaaya, E., and Karlson, P., *J. Insect Physiol.*, **11**, 65 (1965).

¹⁴ Shaaya, E., and Karlson, P., *Dev. Biol.*, **11**, 424 (1965).

¹⁵ Adelung, D., *Zool. Anz.*, **30**, Suppl. 264 (1967).

¹⁶ Adelung, D., *Z. Naturforsch.*, **24b**, 1447 (1969).

¹⁷ Faux, A., and Horn, D. H. S., *Chem. Commun.*, **175** (1969).

¹⁸ Connell, P. A. M., thesis, Tulane University (1970).

¹⁹ Lowe, M. E., Horn, D. H. S., and Galbraith, M. N., *Experientia*, **24**, 518 (1968).

²⁰ Ohtaki, T., Milkman, R. D., and Williams, C. M., *Biol. Bull.*, **135**, 322 (1968).

²¹ Nakanishi, K., Moriyama, H., Fujioka, S., and Koreeda, M., *Science*, **176**, 52 (1972).

²² Van Harreveld, A., *Proc. Soc. Exper. Biol. Med.*, **34**, 428 (1936).

²³ Stevenson, J. R., *Crustaceana*, **14**, 169 (1968).

²⁴ Stevenson, J. R., Guckert, R. H., and Cohen, J. D., *Biol. Bull.*, **134**, 160 (1968).

²⁵ Stevenson, J. R., *Amer. Zool.*, **12**, 373 (1972).

A Salamander Midwife

J. P. A. ANGSEESING¹ is mistaken in most of his criticism of Kammerer's experiments as described by Koestler². Angseesing quite reasonably supposes that the formation of nuptial pads on male midwife toads might involve a genetic mechanism activated by their confinement to aquatic conditions or high temperature. The crux of the argument, however, is the claim that this condition was inherited and intensified in successive generations. Any explanation must therefore involve selection or the inheritance of acquired characteristics, in addition to the probable physiological mechanism.

Contrary to Angseesing's assertions, the colour of newborn salamanders shows no detectable maternal effect, is quite unlike the colour of either parent, and is not likely to be varied by maternal hormones. I have assisted at the delivery of *Salamandra maculosa*, with which Kammerer worked, and so can claim to be a salamander midwife. The embryo of ovoviviparous races develops in an exceptionally stable environment: within a jelly capsule in the uterus of an animal which is hibernating underground, and so relatively unaffected by fluctuations of light and temperature. Even if the appropriate maternal hormones were able to penetrate these barriers to the embryo, which is not known, it seems probable that the amounts of hormone would be remarkably uniform in all cases. Escaping from its capsule within a few minutes of birth, the larva has a mottled grey-brown coloration. It only acquires the brilliant orange-gold and black pattern at metamorphosis, weeks or months later. Kammerer considered that the background caused a corresponding inherited change in the adult pigmentation. Anyone familiar with the variety of adult colour patterns in these salamanders would realize that he could only demonstrate the point by starting with pure-breeding lines, in order to prevent inadvertent selection or genetic assimilation³.

A Lamarckian explanation of these experiments would clearly be important, as indicating a directed mechanism of evolution (and of genetic engineering) which has been generally discounted. The obvious bias in Koestler's book^{2,4} cannot disguise the fact that Kammerer's evidence was inadequate and in one case forged, and therefore discredited. The conception of the experiments, however, was more precise than Angseesing allows. Those of us who cannot read German should be grateful to Koestler, even if we cannot agree with his conclusions.

H. WALLACE

Department of Genetics,
University of Birmingham

Received October 30, 1972.

¹ Angseesing, J. P. A., *Nature*, **239**, 408 (1972).² Koestler, A., *The Case of the Midwife Toad* (Hutchinson, London, 1971).³ Waddington, C. H., *The Strategy of the Genes* (Allen and Unwin, London, 1957).⁴ Wersky, P. G., *Nature*, **234**, 489 (1971).

Elevated Arterial Blood Pressure in an Asymptotic Population of Meadow Voles (*Microtus pennsylvanicus*)

THE factors regulating the growth and final size of mammalian populations include climatic conditions, availability of food, predation, disease, and intraspecific strife¹⁻³. In studies of the demography and organ weight changes in increasing animal numbers in freely growing confined populations of mice, Christian, Lloyd and Davis⁴ have observed that the population growth curve will reach an asymptote after which the population remains close to this size. The attainment of the upper asymptote is associated with cessation of rearing of all young animals. It is not known why reproduction fails at high density but it may involve behavioural changes, failure of lactation and increased intrauterine mortality.

There are many data on the growth rates and organ weights of confined populations but little is known about the physiological status of these animals. In this study a population of meadow voles (*Microtus pennsylvanicus*) was

of new animals present after three consecutive months. The voles were anaesthetized with urethane (1 g kg⁻¹), a carotid artery cannulated with polyethylene tubing (PE 10) and the mean arterial pressure recorded with a 'Statham' strain gauge and 'Gilson' recorder. If any blood loss occurred or the cannula was not patent, the data were not included in the results. Organ weights were determined after fixation in 10% neutral buffered formalin. Animals of approximately the same age distribution, caged as breeding pairs, were used as controls for the animals from the freely growing population, and their blood pressure was measured identically.

The population was terminated after 354 day; the last litter from which any animals survived beyond weaning was born at 270 day. At sacrifice there were twenty-two voles present of which twelve were females and ten were males. Of the females, seven were pregnant but four had resorbing embryos.

Measurement of the blood pressure was successful in fourteen of the twenty-two population animals and seven of the twenty control animals. The mean blood pressure of the population was 121±1.7 (s.e.m.) mm Hg while that from the control animals was 107±2.3. By Student's *t* test for group comparisons, this difference is very highly significant with *P*<0.001. The only significant difference between organ weights observed was that the thymus glands of animals from the population were smaller (0.42±0.05 mg g⁻¹ body wt) when compared to the control animals (0.90±0.10) (*P*<0.001).

Bernardis and Skelton⁵ reported that rats with adrenal regeneration hypertension showed greater increases of blood pressure when housed three per cage as compared to singly caged rats. Henry *et al.*⁶ observed raised blood pressure in crowded mice and these workers, as well as others^{7,8}, have pointed out that social pressures or stress may be involved in the genesis of essential hypertension. My study shows that when animals are allowed to interact until they reach a high stable population density they demonstrate a small, but consistent, elevation in blood pressure. There is evidence that cessation of reproduction may be associated with an altered state of the pituitary-adrenal-gonadal system⁴, and many investigators have also demonstrated a relationship between adrenal steroids and hypertension⁹.

Because thymic involution is a recognized indication of increased adrenocortical function, I suggest that the occurrence of hypertension in asymptotic populations might also be a function of increased adrenocortical function.

I thank Drs J. J. Christian and J. A. Lloyd for valuable discussions. This work was supported by a grant from the US National Institute of Mental Health.

E. H. BLAINE

Table 1 Measurements of Animals from a Freely Growing Confined Population and Caged as Bisexual Pairs

Population				Bisexual pairs			
Sex	Age*	Body weight†	Blood pressure‡	Sex	Age*	Body weight†	Blood pressure‡
M	374	53	120	M	482	46	120
F	374	50	120	F	449	40	112
M	308	64	122	F	378	36	105
F	240	43	110	F	377	39	98
F	195	68	120	M	295	64	112
F	195	69	115	F	293	32	115
F	195	62	117	F	278	44	90
M	178	71	122	M	242	46	122
M	178	51	117	F	220	37	102
F	178	54	125	F	196	36	98
F	178	42	135	F	196	42	110
M	85	55	133	F	161	59	105
F	85	50	120	M	158	42	112
M	85	36	123	M	140	27	100
				F	118	54	95
				M	116	52	115
				F	116	24	120
$\bar{x}$ 121				$\bar{x}$ 107			

* In days. † In g. ‡ In mm Hg.

allowed to reach its asymptotic density and arterial blood pressure was measured. Animals caged as bisexual pairs served as controls.

The population was housed in a stainless steel cage divided into two communicating levels each measuring 2×4.5 foot. Litter material included wood shavings and newspaper placed in the cage at the start of the experiment and not changed, and metal cans were provided as nest sites. Excess food and water were provided, and the lighting, temperature and humidity were controlled. The population was started by introducing one male and two female weanling voles into the cage and leaving them undisturbed except for a monthly census. The population was allowed to grow until reproduction ceased as indicated by a lack

Howard Florey Institute of Experimental Physiology and Medicine,
University of Melbourne,
Parkville, Victoria 3052

Received October 5; revised November 27, 1972.

¹ Christian, J. J., and Davis, D. E., *Science*, **146**, 1550 (1964).² Myers, K., and Poole, W. E., *CSIRO Wildl. Res.*, **8**, 166 (1963).³ Chitty, D., *Can. J. Zool.*, **38**, 99 (1960).⁴ Christian, J. J., Lloyd, J. A., and Davis, D. E., *Rec. Prog. Hormone Res.*, **21**, 501 (1965).⁵ Bernardis, L. L., and Skelton, F. R., *Proc. Soc. Exptl. Biol. Med.*, **113**, 952 (1963).⁶ Henry, J. P., Meehan, J., and Stephans, P. M., *Psychosom. Med.*, **29**, 408 (1967).⁷ Scotch, N. A., and Geiger, H. J., *J. Chron. Dis.*, **1183** (1963).⁸ Henry, J. P., and Cassel, J. C., *Amer. J. Epidemiol.*, **90**, 171 (1969).⁹ Cope, C. L., *Adrenal Steroids and Disease*, 516 (Pitman, London, 1964).

The Ancestry of Birds

WALKER¹ has restated the long-held belief that both birds and crocodiles evolved from thecodont ancestors, but he added the novel suggestion that these two groups arose from a common thecodont ancestor and thus are much more closely related than has been previously realized. Inasmuch as the Thecodontia include the most primitive as well as the most ancient archosaurs known, it is highly probable that all subsequent archosaurs (including birds) were derived from members of this order. Although Walker may be correct, I do not think that the evidence cited indicates such a close relationship between birds and crocodiles as he proposes.

My purpose here, however, is not to challenge Walker's evidence or his interpretation of it. Rather, it is to present (in summary form) other evidence pertaining to the immediate (as opposed to the remote) ancestry of birds—evidence which has generally been ignored for the past 50 years.

The critical evidence of bird ancestry is preserved in the four presently known specimens of *Archaeopteryx*, which occupy a position much closer to avian origins than do the Triassic thecodonts mentioned by Walker. Before we can trace the remote origin of birds among thecodonts, we must be certain that we have correctly assessed the evidence in those specimens pertaining to the immediate ancestors of *Archaeopteryx*. With this in mind, during the past two years, I have studied all four *Archaeopteryx* specimens and currently I am preparing a detailed paper on the origin of birds. Walker's paper, however, has prompted this preliminary note, for in my opinion his theory can only be valid if it is totally consistent with a thecodont-coelurosaur-*Archaeopteryx*-Aves phylogeny. The skeletal anatomy of *Archaeopteryx* is almost entirely that of a coelurosaurian dinosaur—not thecodont, not crocodilian, and not avian.

The following coelurosaurian features of *Archaeopteryx* collectively are here considered as *prima facie* evidence of a coelurosaurian (Theropoda) ancestry of birds:

(a) Vertebral column: (1) Thoracic vertebrae pleurocoelous (and probably amphicoelous). (2) Ten cervical vertebrae and 12 to 15? thoracics.

(b) Fore limb: (3) Manus reduced to digits, I, II and III. (4) Phalangeal proportion of the fingers. (5) Proportions of the three metacarpals. (6) Carpus of two or three elements including a lunate radiale. (7) Proportions of humerus to radius and ulna. (The fore limb is not reduced in all theropods as has been frequently claimed; see for example *Ornitholestes*, *Velociraptor*, *Deinonychus*, *Ornithomimus* and *Deinocoelurus*.) (8) Morphology of the humerus.

(c) Pectoral arch: (9) Very narrow, strap-like scapula. (10) Subrectangular coracoid fused to scapula.

(d) Hind limb: (11) Pes with four digits, V being lost. (12) Phalangeal proportions. (13) Reversed hallux. (14) Metatarsal proportions. (15) Mesotarsal joint. (16) Well developed ascending process of the astragalus. (17) Hind limb proportions. (18) Morphology of the femur.

(e) Pelvis: (19) Shape of the ilium. (20) Shape of the pubis, with a distal expansion and a long symphysis. (21) Open acetabulum.

In addition, it is possible that the pubis of the Berlin specimen (apparently preserved in the avian position) is dislocated and that it was originally directed ventrally or antero-ventrally as in theropods². The proximal portion of that pubis is damaged, and the pubis of the Teyler specimen is oriented nearly perpendicular to the axis of the posterior thoracic vertebrae rather than obliquely as in the Berlin specimen.

Certain other non-avian characters present in *Archaeopteryx*, although perhaps primitive in origin are also typical of coelurosaurs. These are: (1) Long, unfused caudal series numbering at least 20 segments and apparently with elongated zygapophyses and chevrons. (2) Presence of gastralia. (3) Thecodont dentition. (4) Antorbital fenestra. (5) Probable presence of an external mandibular fenestra.

Many of these characters have been noted before, but two very important features have not. First, the lunate form of the radiale in *Archaeopteryx* has only recently been recognized in theropods (namely *Deinonychus*³, *Stenonychosaurus*⁴ and a recently collected specimen probably referable to *Velociraptor* (Kielan-Jaworowska, personal communication)). Second, the astragalus in *Archaeopteryx* has a well developed ascending process that apparently has not been noticed before, even though it is conspicuous in both the London and Berlin specimens.

These "coelurosaurian" characters of *Archaeopteryx* have in the past been attributed to parallel or convergent evolution, but the large number of features involved, and the complex nature of many of them, make this highly improbable. In my opinion, common ancestry alone cannot account for the overall coelurosaurian nature of *Archaeopteryx*.

No one any longer doubts the avian identification of these specimens—or their significance for the origin of birds, presumably because of the remarkable feather impressions. Yet they possess only two osteological characters that are exclusively avian, the furcula (preserved in the London and Maxberg specimens) and the possibly reverted pubis (doubtfully preserved in natural articulation only in the Berlin specimen). Indeed, if feather impressions had not been preserved all *Archaeopteryx* specimens would have been identified as coelurosaurian dinosaurs. The only reasonable conclusion is that *Archaeopteryx* must have been derived from an early or mid-Jurassic theropod.

A dinosaurian origin of birds is not a new idea, but it has been widely dismissed for the last 50 years because Broom⁵ and Heilmann⁶ concluded that coelurosaurs were not sufficiently primitive. The sole anatomical evidence cited by Heilmann for rejecting a coelurosaurian ancestry for *Archaeopteryx* was the absence of clavicles in all known theropods. Clavicles have been reported, however, by Camp⁷ in *Segisaurus*, and by Osborn⁸ in *Oviraptor*; furthermore a specimen, probable referable to *Velociraptor*, recently collected by a joint Polish-Mongolian expedition to the Gobi Desert (Kielan-Jaworowska, personal communication) also appears to possess clavicles. Regardless of these few occurrences, however, the absence of clavicles in theropods is only negative evidence and, in view of the fact that the clavicle is dermal in origin, its absence in fossil specimens has no phyletic significance. It may well have been membranous (but not lost) in most theropods and thus not preservable.

The most likely origin of so many coelurosaurian features in *Archaeopteryx* is by direct inheritance from a small coelurosaurian ancestor. The additional significance of this phylogeny is that "dinosaurs" did not become extinct without descendants and I suggest that feathers, as thermal insulators, could be the primary reason for the success of dinosaurian descendants. Can it be just coincidental that mammals succeeded as therapsid descendants (at least partly) because of a comparable adaptation—perhaps acquired at about the same time?

J. H. OSTROM

Department of Geology and Geophysics,
and Peabody Museum of Natural History,
Yale University,
New Haven, Connecticut

Received September 11; final revision December 26, 1972.

¹ Walker, A. D., *Nature*, **237**, 257 (1972).

² Ostrom, J. H., *Proc. Koninkl. Neder. Akad. Wetensch.*, B, **75**, 4 (1972).

³ Ostrom, J. H., *Bull. Peabody Mus. Nat. Hist.*, **30** (1969).

⁴ Russell, D. A., *Canad. J. Earth Sci.*, **6**, 595 (1970).

⁵ Broom, R., *Trans. S. Afr. Phil. Soc.*, **16**, 355 (1906).

⁶ Heilmann, G., *The Origin of Birds* (Appleton, New York, 1927).

⁷ Camp, C. L., *Bull. Geol. Sci. Univ. California*, **24**, 39 (1936).

⁸ Osborn, H. F., *Amer. Mus. Nat. Hist. Novitates*, **144**, 1 (1924).

BOOK REVIEWS

Evolution of Geology

Charles Lyell: the Years to 1841: The Revolution in Geology. By L. G. Wilson. Pp. xiv + 553. (Yale University Press: New Haven and London, 1972.) £7.50.

CHARLES LYELL was one of the founders of modern geology and, in his time, one of the greatest expositors of the subject. He, more than any other nineteenth century geologist, was responsible for removing the subject from the realm of speculation and conjecture, and establishing it on a sound philosophic basis. Lyell was also the friend, counsellor and confidant of Charles Darwin. Sir Archibald Geikie remarked many years ago that it is apt to be forgotten that Darwin started his scientific career as a geologist; and more recently the American author Loren Eiseley pointed out that the world tends to forget that Lyell wrote extensively on zoological subjects. The relations between Darwin and Lyell add materially to the interest of this new book.

Lyell's interest in geology was first aroused when, as an undergraduate at Oxford, he attended the lectures of William Buckland. After leaving Oxford in 1819 he studied and practised law for a few years, but finally gave up law to devote his life entirely to the advancement of geological science.

As an original observer, Lyell gained recognition quite early in his career; and the publication of his famous *Principles of Geology* (1830–33) greatly enhanced his reputation. In this work he defined geology as "the science that investigates the successive changes that have taken place in the organic and inorganic kingdoms of nature". The inclusion of organic nature in this definition is noteworthy. As a student of stratigraphy Lyell very soon became aware of the remarkable changes in animal life, exemplified by fossils, that marked the passage from past geological ages to the present day. Before Darwin came on the scene, Lyell had already given much thought to the problems raised by these changes.

The method Lyell used in carrying out his geological investigations is sum-

marized in the sub-title of the *Principles* as "An attempt to explain the former changes in the Earth's surface by reference to causes now in operation". This method, later known as the principle of uniformitarianism (the name was coined by William Whewell), had been foreshadowed before 1800 by James Hutton (to whom Lyell made an acknowledgment), but in the *Principles* it was advocated much more clearly and convincingly, and was supported by a wealth of evidence that had not been available to Hutton. Gradually, though not without opposition, the principle of uniformitarianism came to be accepted by other geologists. It prepared the way for acceptance of Darwin's ideas about evolution, and before the end of the nineteenth century it had been adopted generally as the guiding principle for all geological research. Although Lyell advanced geological science in many other ways, this was his most important contribution to the subject.

Lyell realized the necessity for a geologist to widen his experience by travel, and he became an inveterate traveller, journeying to many parts of Europe and to North America. During these tours he consulted local geologists and exchanged information and ideas with them. Even during a honeymoon tour in Europe he took the opportunities offered to examine rock sections. Although he was a dedicated geologist, Lyell had other interests: history, politics, social problems and education, and for nearly fifty years he occupied a prominent position in both intellectual and social circles in Great Britain.

Hitherto the only detailed account of Lyell's life has been the two volume *Life, Letters and Journals of Sir Charles Lyell Bart.* (1891), edited by his sister-in-law Mrs [Katherine] Lyell. This is a mine of information, but there are gaps in the record, and the work makes no pretence to supply a critical account of Lyell's contributions to geology. Three short biographical accounts of Lyell's career have appeared since. T. G. Bonney's *Charles Lyell and*

Modern Geology (1895) was written by a geologist whose career overlapped that of Lyell, and it provides a nineteenth century assessment of Lyell's personality and his contributions to geology; E. B. Bailey's *Charles Lyell* (1962) is a much condensed version of Mrs Lyell's book, with added geological commentaries; while F. J. North's *Sir Charles Lyell: Interpreter of Modern Geology* (1965) is a brief account suitable for students and general readers. Lyell's importance as one of the leading scientists of the nineteenth century certainly justifies a new and more systematic account of his life and work; and Professor Leonard G. Wilson of the University of Minnesota is engaged in producing such a biography.

The author has been at work on this book for about ten years, and plans to complete it in three volumes. In preparation for the task, he first made an extensive search for new material, particularly for unpublished sources in archives in Great Britain, North America and elsewhere, and his search has justified the effort involved. The most important archive proved to be the mass of family papers preserved in Lyell's old family home in Scotland, Kinnordy House, Angus. Here, in addition to a very large number of family letters, often containing matter of geological as well as biographical interest, the author found Lyell's field notebooks and travel diaries, containing much information that had not been used by Mrs Lyell. The present representatives of the Lyell family generously allowed him complete freedom to examine and use for publication any material he thought suitable.

An important early find was the set of seven "Scientific Journals", in which Lyell recorded his private thoughts on the question of the transmutation of species, from 1858 to 1861, that is, the period immediately preceding and following publication of the *Origin of Species*. The length and importance of these journals justified separate treatment, and they have already been published¹. A second important source

was the collection of letters received by Lyell, now in the library of the American Philosophical Society, Philadelphia. This archive contains 177 letters from Darwin to Lyell (most of which appear to have been published), and also 270 letters to Lyell from prominent geologists and other scientists. In passing, one cannot help expressing regret that the export of this great collection, sold at auction in 1950 for £5,200, was not prevented at the time, on the ground that it formed part of the British national heritage, an appellation which seems more appropriate for this collection than for some of the paintings about whose export there has been so much concern in recent years.

Professor Wilson has recorded his failure to trace all but a few of the large number of letters which Lyell must have written to Darwin, and concludes that they have not survived². The explanation seems to lie in a statement made by Francis Darwin that, though Darwin's habit was to file letters, owing to his small stock of files, when these became full he burnt the letters of several years to free the files for further use; and this habit resulted in the destruction of nearly all letters received before 1862.

The first volume of Professor Wilson's work, which has the sub-title "The Years to 1841: the Revolution in Geology", takes Lyell's career up to that year. The author's term "revolution" clearly refers to the immense changes in geological thinking brought by the publication of the *Principles*. By 1841 geology was certainly on its way to becoming a science guided by reason rather than speculation, but it can hardly be claimed that the revolution was complete.

The author's treatment of his subject is chronological, but with the emphasis on Lyell's development as a geologist, the evidence that led him to formulate the principle of uniformitarianism, and his concern with the biological aspects of geology. An early chapter, entitled "The State of Geology in 1819", prepares the reader for what is to follow. Later chapters deal with more specific incidents in Lyell's career.

Thus, chapter 13 describes his relations with Darwin up to 1841. When, in October 1836, Darwin returned from the voyage of the *Beagle*, he was much concerned with the geological results obtained during the voyage. Lyell met him soon afterwards, and secured his election to the Geological Society on November 30, 1836. The two, having much of mutual interest to discuss, soon became close friends. Darwin's contacts with the palaeontologist Richard Owen are also discussed in this chapter.

Chapter 14 relates Lyell's involvement in the question of the age of the

Crag (Tertiary) deposits in East Anglia. In assigning relative ages to particular beds he had reached his conclusions mainly by calculating the percentage of fossil shells present in each bed could be matched by forms still living in adjacent seas; and his conclusions had been criticized by local geologists. Lyell realized that the general application of this method required a skilled knowledge of conchology, and also general agreement as to what criteria should be used in defining species. Differences existed at that time between acknowledged experts in this field. It was typical of Lyell's thoroughness that in 1837 he made a prolonged visit to Copenhagen to study the species question under one of the experts, Dr Beck.

Chapter 15 discusses Lyell's reception of the new and revolutionary Glacial Theory announced by Louis Agassiz in 1840. Agassiz had demonstrated to Buckland evidence of the former presence of glaciers in Great Britain, and Lyell in turn was convinced after Buckland had shown him the field evidence. When, late in 1840, Buckland and Lyell jointly announced to the Geological Society their acceptance of Agassiz's Theory it created a sensation. Buckland informed Agassiz that Lyell had accepted his Theory "in toto", but, as Professor Wilson remarks, a long time passed before Lyell and his contemporaries accepted the wider implications of the glacial theory. The incident does, however, illustrate one aspect of Lyell's character, his willingness to accept from others new ideas, once convinced by the evidence.

As an example of how hard and conscientiously Lyell worked, it is worth mentioning that by 1840 the *Principles* had reached a sixth edition, each edition having been thoroughly revised; while in 1838 he had published a new work, his *Elements of Geology*, the first modern textbook for students of the subject. In a letter to his father, written in 1840, Lyell, referring to the publication of new editions, stated, "It is necessary to resist the temptation of coining money by simply compiling material provided by others instead of testing their accuracy by personally examining the facts in the field wherever they are easily come-at." Lyell was not an armchair geologist.

In July 1841, Lyell, accompanied by his wife, set out for America, where he remained a year. This journey was something of an adventure, and the present volume concludes with a letter to Lyell from his father urging him to take care of the health both of himself and his wife. Lyell was fortunate in having a father who encouraged his scientific aspirations and supported him financially when necessary (he eventually earned considerable sums from his

books). His wife, too, shared with him the pleasures and hardships of many of his geological trips, on one occasion entertainment by the royal family in Denmark, but more often the discomforts commonly met with on geological field trips.

The first volume of this biography is a most interesting book, written in an unpretentious and readable style. It is illustrated with a number of contemporary portraits, and sketch maps of Lyell's tours; and it contains an extensive list of sources and an adequate index. In preparing it Professor Wilson has clearly been to great trouble to search out, collate and assimilate the large amount of source material available, much of it new. He makes no attempt to provide in this volume a detailed and critical assessment of Lyell's many contributions to geology, but he relates the part Lyell played in the gradual introduction of new ideas in the formative period of what was then the new science of geology, and his contacts with his geological contemporaries and other prominent scientists of the period. When completed this biography will provide valuable source and background material for students of the history of science in the nineteenth century, and it must certainly appeal to any geologist interested in the history of his subject.

V. A. EYLES

¹ *Sir Charles Lyell's Scientific Journals on the Species Question* (edit. by Wilson, L. G.) (Yale University Press, New Haven and London, 1970).

² *Life and Letters of Charles Darwin* (edit. by Darwin, F.), 1 (London, 1887).

The Story of Marconi

Marconi: a Biography. By W. P. Jolly. Pp. 292+24 photographs. (Constable: London, October 1972.) £3.95.

AN account of Marconi's early life inevitably reads like a compendium of biographical clichés. He was fourteen years old, and a scholastic failure, when Hertz discovered radio waves, yet he became the first person to harness them for communication. His father disapproved of his early experiments but his mother believed in him, and came with him to England when the authorities in his native Italy refused to sponsor him; within eighteen months he was back, a celebrity. He transmitted a radio signal across the Atlantic when established wisdom said it should have flown out into space, and he won a Nobel prize for physics after failing to gain entry to university.

Professor Jolly's impressively researched biography brings depth to this story without sacrificing readability, and his perceptive analysis makes Marconi's achievements credible without diminishing them. Radio in the 1890s, involving

a small number of imperfectly understood variables, is seen as being susceptible to empirical improvement by a resourceful and determined experimenter with no theoretical interest to distract or inhibit him. There are generous digressions to impart background information, and it is only very occasionally that these irritate by straying too far from the point.

Money was not of primary interest to Marconi, but the company that he founded to further the development of "wireless telegraphy" was often criticized for its aggressive business methods. Professor Jolly comments, with characteristic pithiness:

"... the fact that the company was based on scientific innovation did not relieve it of the need to advertise, to impress investors, and to confuse competitors. Such ordinary commercial requirements precluded running the company as though it were some sort of sub-branch of a learned society, which was apparently what many critics of its methods wished."

Whatever the ethics of the Marconi Company, the author establishes from its archives, and from those of the Public Record Office, that both the Post Office and the Admiralty countered by engaging in decidedly sharp practice.

The success of the company was, of course, largely due to the work of its engineers, and readers familiar with the history of radio will be disappointed to find that such distinguished men as C. S. Franklin and H. J. Round receive little mention in this book; the reader may overcome this omission by referring to W. J. Baker's *A History of the Marconi Company* (1970), but it is nevertheless regrettable. Though technical matters tend to be played down, they are lucidly dealt with, thanks to the author's professional background in electronics. There are, however, two unfortunate slips: Hertz's transmitting apparatus is incorrectly described, whilst early long-range telegraphy is stated to have used wavelengths of "hundreds of metres", whereas in fact wavelengths of several thousand metres were always employed.

The book's longest chapter is devoted to the "Marconi scandal" of 1912-13. This concerned completely unfounded allegations that a Government contract had been corruptly awarded to the Marconi Company, and distorted allegations (though with some basis of fact) that ministers had used their privileged position to speculate profitably in Marconi shares.

Marconi himself was concerned only insofar as his name was inevitably besmirched, but the affair caused a great outcry and came close to wrecking Lloyd George's career. This complex story is skilfully summarized.

The years after 1918 saw one of

Marconi's greatest triumphs—the development of the short-wave "beam" system—yet it was basically a period of decline. Many distinguished people paid social visits to Marconi's luxury yacht, which was also his floating laboratory. Lord Mountbatten, then a specialist in radio, was one of those able to ask his host specific questions, and is quoted as recollecting: "The impression I had was that at this stage, which I think was about 1926, he had lost close contact with all the developments, although they were being carried out under his aegis."

Marconi was a shy man, unexcitable in spite of his Irish/Italian parentage; in later life he became increasingly withdrawn, and no attempt is made to present him as a colourful personality. Neither is great emphasis given to the long, slow decay of his first marriage, or to his numerous love affairs, though they are treated with sensitivity and insight.

Professor Jolly has gathered much valuable material, particularly from the Marconi Company's own archives. Notwithstanding its considerable scholarship, the book is written in a straightforward, unforced style, and can thus be recommended to specialist and general reader alike.

KEITH GEDDES

How to Grow Crystals

Laboratory Manual on Crystal Growth. Edited by I. Tarjan and M. Matrai. Pp. 250. (Akademiai Kiado: Budapest, 1972.) £4.

THE modest title of this book indicates accurately its contents; it is a collection of recipes for experiments which either demonstrate various crystal growth phenomena or enable the reader to grow single crystals destined for applications based on their special properties. It is indeed an unusual book; I do not know another like it on this subject. It collects together the sort of notes that are usually compiled in individual laboratories and are duplicated for students' use. Here we have many recipes, by nineteen contributors, arranged so as to form a practical course in crystal growth. The need for such a book has often been mentioned at crystal growth conferences; the present volume aims to satisfy that need. Any individual laboratory may have its own programme of experiments covering a limited range of experience; here the experiences of many people, covering a wide range of phenomena and methods, are collected together for the convenience of teachers and students.

The first half of the book deals with basic phenomena, and gives detailed

instructions for many experiments aimed at familiarizing the reader with the conditions for the spontaneous formation of crystal nuclei and the growth of various types of crystals: dendrites and whiskers as well as the usually more desirable polyhedra. Growth from solutions, melts and vapours, epitaxial growth of one species on another, the detection of dislocations by etching, and recrystallization in the solid state, are among the topics dealt with. The book does not try to deal seriously with attempts to understand the phenomena in terms of molecular processes, but each section starts with an introduction to give some sort of theoretical background for the experiments. Such introductory material is very necessary to provide a conceptual framework, and these paragraphs are for the most part soundly based; but some statements are questionable. For instance, the spreading of new layers from the corners of crystals growing rapidly in highly supersaturated solution is said to support Kossel's notion that layers start where energy release is greatest; actually Kossel only claimed the operation of this principle at very low supersaturation; at high supersaturations, convergent diffusion at the corners is sufficient explanation.

The phenomena dealt with in this section of the book are all concerned with the growth of individual crystals. The problems of bulk growth, which involve the number of spontaneously formed nuclei as well as their subsequent growth, are not considered at all. The influence of supersaturation and the presence of other substances might well form the subjects of instructive experiments which would make this course on crystal growth more comprehensive.

The second half of the book contains detailed instructions for growing large crystals by the various methods now in general use, from solutions, from melts by zone melting or pulling from the melt, and from vapours. The apparatus necessary is described in detail and the precise instructions on quantities and procedures are evidence that the information is based on first-hand experience. The book ends with some pages on the processing of crystals: cutting and drilling, grinding and polishing.

The advantages of bringing together contributions by many authors, each having first-hand experience in a particular field, are obvious. Such a book may lack consistency of style, and is frankly a string of recipes; but its value as a practical laboratory manual is great. It should be in the hands of all teachers and students of crystal growth, for it covers most of the practical aspects of the subject; the one

notable omission is a section of quantitative experiments on spontaneous nucleus formation. C. W. BUNN

Physiology of Breathing

Comparative Physiology of Respiration. By John D. Jones. Pp. vi+202. (Edward Arnold: London, 1972.) £4 cloth; £2 paper.

OF all branches of comparative physiology, respiration has a special appeal to biologists. This is partly because of the great diversity of evolutionary adaptations, their range in vertebrates being wider in structure and function than those in, for example, the nervous or the cardiovascular system. Secondly, man may find himself in a variety of extreme conditions and environments in which it is the limitations of breathing which are the most obtrusive—in exercise, high altitude, diving, pollution and asphyxiation, and even in hot climates and outer space. This is presumably because the small size of our reserves of respiratory gases and the rapid effects of alterations in gas exchange cause more serious deterioration than does restriction of other bodily activities. In these conditions we may envy our animal cousins with different adaptations, and try to emulate them by artificial means. The study of the comparative physiology of respiration has flourished in recent years. The research has direct relevance not only to man in unusual conditions, but also to respiratory illness where our adaptations cannot cope adequately with disease processes.

Dr Jones's book never fails to excite interest in the zoological adaptations of respiration. It is avowedly teleological, and whether this is a fault or not can be argued. Oddly enough, teleology can damp down speculation, for if every adaptation is appropriate there is no need to look for deficiencies and disadvantages. Joseph Barcroft's attitude to teleology was complementary and may be preferable: that of noting the nature and degree of functional impairment that an evolutionary specialization may carry with it. The book is also deliberately biased in favour of respiratory pigments and gas transport to the neglect of mechanisms of breathing and tissue respiration. Strictly speaking, the word respiration is more applicable to the latter two processes.

These approaches are legitimate author's privilege. What is presumably not intended is the lack of up-to-date information on some topics. Acclimatization to high altitude has been profitably studied since 1964, the latest reference; foetal and neonatal respiration, surely an important aspect of comparative physiology, is almost ignored,

as are recent studies on the nervous control of breathing and the mechanics of the respiratory apparatus. Some sections are inaccurate or confused; for example, the definitions of vascular resistance, the description of oxygen "buffering" by haemoglobin, and the role of peripheral chemoreceptors. There are errors in the references.

Some of these are small criticisms, and all may be corrected. The great merit of Dr Jones's book is that it arouses interest and provokes thought. Undergraduates, research workers and teachers will be stimulated and educated by it. J. G. WIDDICOMBE

Semiconductors

Semiconductors and Semimetals. Edited by R. K. Willardson and Albert C. Beer. Volume 9. *Modulation Techniques.* Pp. xiii+574. (Academic: New York and London, June 1972.) \$29.50.

It is often said that research on conventional semiconductors such as Ge, Si or GaAs is nowadays merely a matter of dotting the i's and crossing the t's. Be that as it may, techniques are still emerging from work on these materials which are important in that they have wide applicability to the study of solids in general. This book deals with an excellent example of such a technique, namely the use of modulation methods for observing weak structure in the optical spectra of solids. Although the high sensitivity of modulation methods has been known to physicists for a very long time, they have only recently applied them in this way. The motivation has been an improved knowledge and understanding of the energy band structure of solids beginning with semiconductors.

Study of the band edges either side of the fundamental energy gap of a semiconductor is eminently feasible by straightforward transmission measurements such as absorption, magneto-absorption, cyclotron resonance or Faraday rotation. However, at energies appreciably removed from the energy gap, the transmission becomes too small and recourse must be made to reflexion experiments. Unfortunately reflexion spectra are relatively diffuse, but the use of thermal, stress, electric or wavelength modulation lifts weak but sharp spectral features associated with critical points from the diffuse background. Assigning structure to particular critical points and hence determining their energies is assisted by the presence of symmetry breaking parameters either inherent in the modulation method or separately applied statically.

This ninth volume in the series *Semiconductors and Semimetals* will presumably find most usage as a source

of reference. It will be a welcome addition to the bookshelves of research workers in the field. Most of the chapters are written by authors who have established international reputations; they include B. O. Seraphin, R. L. Aggarwal and I. Balslev. The physical content is generally of a high standard but the editors have failed to eliminate a great deal of repetition amongst the authors. Almost 1,000 references are included and a serious attempt has been made to bring the volume up to date by the inclusion of addenda added at the proof stage. Even so, developments are only covered up to and including 1970 and, inevitably I suppose, the book is already two years behind "the state of the art". Minor criticisms include excessive verbosity in the first chapter and a glib treatment of experimental aspects in places.

In conclusion, a major criticism of the series *Semiconductors and Semimetals* is the narrow coverage of material within the volumes published so far. They are almost exclusively about III-V compounds. Treatment of such topics as narrow band (hopping) and amorphous semiconductors is long overdue and semimetals should be given more emphasis to justify the series title. A. K. WALTON

Muscle Regenerates

The Regeneration of Minced Muscles. By Brice M. Carlson. Pp. vii+128. (S. Karger: Basel, London and New York, 1972.) Sw. francs 49; £5.40; \$13.75.

THIS book is volume four in the *Monographs in Developmental Biology* series published by S. Karger under the editorship of A. Wolsky. It is a short (128-page) lucid account of a field of work in which the author has already established himself as a leading authority.

As well as describing his own work Dr Carlson describes the earlier Russian work on which his own observations are based and also gracefully acknowledges the as yet unpublished work of some of his graduate students.

While the book is dominantly histological in character considerations of gross anatomy, biochemistry and functional characteristics of muscle regenerates are discussed. The author's style is clear and direct, and the standard of illustrations is high.

A final chapter, entitled "Epilogue", poses several interesting questions for future study, and discusses the potential use of minced muscle techniques in traumatic surgery.

Unfortunately this slim volume is priced at £5.40, but even so is likely to be the authoritative work on the subject for some years to come.

A. J. BULLER

Inflammatory Substances

Chemical Mediators of the Acute Inflammatory Reaction. By M. Rocha Silva and J. G. Leme. In collaboration with Hanna A. Rothschild. Pp. ix+263. (Pergamon: Oxford and New York, December 1972.) £6.

FELDBERG and his colleagues in the 1930s established histamine as a chemical mediator of inflammation. Since then, other possible components of the inflammatory response have been found, including 5-hydroxytryptamine, slow reacting substance in anaphylaxis (SRS-A), bradykinin, various chemotactic factors, rabbit aorta contracting substance (RCS) and prostaglandins of the E and F series. Each substance, as its involvement is proposed or demonstrated, has been regarded as the most important inflammatory mediator, only to be supplanted after a few years by the latest pharmacological fashion.

During the past few years we have passed from the bradykinin phase into the prostaglandin phase of inflammation, but this change of emphasis is not reflected in this book; perhaps partly because bradykinin was discovered and named by one of the authors, but mainly because there must have been an inordinate delay between writing the manuscript and eventual publication. This is sad, for in such a fast-moving field, as inflammation, it means the book is already out of date. For example, of more than 400 papers which the authors cite in the main chapter on chemical mediators, only forty-six were published in the last four years and sixteen of these came from their own laboratory. Thus, little cognizance is taken of work published since 1968 by Solomon and Juhlin, Kaley and Weiner, Crunkhorn and Willis, Spector and Willoughby, Greaves and his colleagues and Ånggård and Jonsson, involving prostaglandins in the inflammatory response and that published in June 1971 from my laboratory, demonstrating that inhibition of prostaglandin biosynthesis may be the fundamental mechanism of action of aspirin and similar anti-inflammatory drugs.

The book consists of four chapters, each one of which is well written, fluent and lucid. The first deals with the history of the inflammatory reaction. The second describes methods of measurement of the inflammatory response and concentrates on techniques which quantitate vascular leakage by extravasation of dyes or carbon particles. It would have been useful if these methods could have been compared with the ones using extravasation of labelled large molecules as a quantitative method. Co-axial perfusion of tissue spaces as a means of collecting potential mediators of

inflammation is also given considerable space, but the important work of Lewis and his colleagues, who measure enzymes and other chemicals in the lymph draining an injured site, is not recounted.

The third and largest chapter concerns the chemical mediators and quite naturally devotes considerable space to arguing the case for the participation of bradykinin. The last chapter deals with the anti-inflammatory drugs and demonstrates well the difficulties encountered in proposing a mechanism of action for these substances before it was discovered that they prevented formation of prostaglandins.

The book, then, is a useful reference source for those interested in reading about the history of chemical mediation of inflammation up to 1968. Work published since then is not adequately reviewed.

J. R. VANE

Sound in Hologram

An Introduction to Acoustical Holography. By B. P. Hildebrand and B. B. Brenden. Pp. xii+224. (Adam Hilger: London, August, 1972.) £7.

THE purpose of this book, as stated in its preface, is an admirable one, namely, "To bring together the results of research in acoustical holography so that workers in non-destructive testing, medical imaging, underwater imaging, and seismic exploration can decide whether this new technique can be useful to them". The wave of interest which followed the publication in 1966 of the first papers on this subject brought with it not a few misunderstandings and fanciful claims concerning the results which were thought by some to be possible. An authoritative review of this kind is therefore to be welcomed. The text of the book is clear and concise, useful lists of references are included for further reading and there are more than eighty illustrations of actual holograms and reconstructed images, some of them published here for the first time, which have been obtained by the various methods described.

A short introductory chapter shows how the original Gabor concept of holography led to the Leith-Upatnieks system, in which the directions of the interfering beams are chosen to facilitate the suppression of the unwanted image. Chapter 2 gives the basic theoretical analysis of holography including the replacement of the reference beam by an electronic reference. Chapter 3 is devoted to a brief summary of the theory of the propagation of acoustic waves in liquids and solids, refraction, reflexion, radiation pressure and so on. There follows a detailed analysis of various ways in which a hologram can be built up by, for example, using a moving point

receiver to scan the field by measuring the phase and amplitude at each position and then using the signal to modulate a synchronously-scanned light source over a photographic film. The analysis includes the calculation of the angular resolution of scanned holograms and of the aberrations occurring at high scanning velocities. The use of time gating, as an additional means of rejecting unwanted data from particular regions of object space, is also mentioned. There is a chapter on the application of information theory to sampled holograms and results obtained with a circularly-scanned system are used to illustrate the conclusions reached. The theory of the interaction of sound and light at a liquid surface is described and applied to an important direct method of hologram formation. Other methods, such as the use of thermoplastic film as a detector and the optical heterodyne technique, are briefly described. Finally, some applications of acoustical holography are described. These range from well-established fields such as medical diagnostics and non-destructive testing to more tentative proposals relating to underwater acoustics and seismic exploration.

V. G. WELSBY

Crystal Spectra

Infrared and Raman Selection Rules for Molecular and Lattice Vibrations: The Correlation Method. By William G. Fateley, Francis R. Dollish, Neil T. McDevitt and Freeman F. Bentley. Pp. 222. (Wiley: New York and London, November 1972.) £5.45.

IN recent years there has been increasing interest in the analysis of the vibrational (infrared and Raman) spectra of crystals. This has been something of a "no man's land" between the normal territories of the spectroscopist and the crystallographer with, for the former, unusual notations and pitfalls to be overcome. Although the most direct approach to the analysis of crystal spectra—the correlation method—was developed more than twenty years ago by Hornig and Halford and their co-workers, the results of this method have not been systematically tabulated, with the result that a number of misunderstandings have led to mistakes in the literature. The aim of this short book is to present systematically the tabulated data and to apply these in detail to a sufficient number of examples to make clear the correct way of using the method and its scope. This is well and clearly done. The only criticism of the book is that it is very much a spectroscopist's publication dominated by the Schoenflies point group notation with which the reader is expected to be fully familiar. As crystallographers are

becoming increasingly interested in crystal dynamics it would have been advantageous to both sides if the relationship between the Schoenflies and Hermann-Mauguin symbolism had been made clear, the translation-containing symmetry elements of space groups described, and a discussion given of the derivation of the isomorphous point group from the symmetry elements of the crystallographic space group. However, this would have been mainly an educational gain, as for most particular examples the necessary relationships are to be found in the comprehensive tables.

The high price of this small book may be the result of the extended tabulated matter it contains. However, it is a very useful handbook which should certainly be on the bookshelves of all interested in the vibrational spectra of crystals, and a number of other spectroscopists besides.

N. SHEPPARD

Denaturation Dangers

Certainty and Uncertainty in Biochemical Techniques. By Harold Hillman. Pp. x+126. (Surrey University: Henley-on-Thames, October 1972.) £2.85.

EVERY argument used in science has its loopholes, and some of the most dangerous are those easily forgotten because the arguments have ceased to be stated explicitly because they refer to popular techniques. Hence a book that aims to scrutinize the assumptions of such techniques is welcome. In biology care must be taken to compare results at different levels of study, and the danger in the more controlled situations of the biochemist is that they are "unphysiological", frequently because the experimenter may alter his material in ways of which he is not aware. The author examines the six techniques of subcellular fractionation, histochemistry, electron microscopy, measurements of radioactivity, electrophoresis and chromatography. After this he makes some general comments and summarizes his conclusions, largely that "an experiment is only as good as its controls".

Although several of the real difficulties of using and interpreting each selected technique are given, and several wise comments are spread throughout the book, its overall approach is largely spoilt by the author's concentration on heat. Because heating can harm biological material, each experimental situation is assessed largely in terms of the heating that occurs. Even that produced by impact of subcellular particles on the bottom of the centrifuge tube is

reckoned one of the dangers of subcellular fractionation.

Of course, the author is right that a protein may be denatured by heat during its extraction and that a procedure that keeps it cool diminishes this risk. But much can be done to assess whether the changes feared have occurred, by examining the homogeneity of the product and by comparing proteins extracted in different ways. This will not provide a complete answer, for a "denaturation" could involve the quantitative conversion of a protein into a single altered form. In the book such tests are little discussed and the reader is left with repeated warnings of the dangers of local heating and the difficulties of assessing its degree.

Electrophoresis seems to be a case where the author's concentration would be justified because most of the difficulty of scaling it up is the dissipation of heat. The rate of separation depends on the potential gradient, and the conductivity of the medium must be kept high because otherwise the substances being separated would significantly affect this conductivity. The potential gradient would then differ between the centre and edges of each band, and this would result in distortion and spread of each peak. But the author does not consider this, and discusses electrophoresis in terms of the differential heating of different bands. The controls of checking that the mobility remains constant, both with time and with potential gradient up to the highest used, are not discussed, let alone re-running of the fractions. I am not trying to deny the danger of overheating; indeed I have myself turned up the potential too high and seen a spot of haemoglobin that had been running nicely stop dead as it denatured. But here, as throughout, more emphasis on the possible checks, and less repetition of how unknown and dangerous heating effects are, would have given the book greater balance and value.

H. B. F. DIXON

Engineering in Context

Creativity and Innovation in Engineering. Edited by S. A. Gregory. Pp. 313. (Butterworth: London, 1972.) £5.

ANY word in the engineer's vocabulary that is associated with design seems doomed to ambiguity. For example, if you find the words creativity or innovation on the cover of a book you cannot tell what they are intended to mean until you look inside. Chameleon like, they take their colour from the leaves in which they are hidden. There is a whole spectrum of possibilities. At one extreme you find the enthusiastic

designer regarding creative work only meaningfully portrayed in the work that has been created, that is, the final structure or machine. To him the elegance of the design and its embodied mathematics is the test of creativity. His book would be full of diagrams and calculations, for drawings would be his prose, mathematics his grammar, and differential equations his poetry. At the other extreme would be a book totally concerned with the context of designing, that is, the educational background, the commercial foreground and environmental constraints within which the designer must design. The spectrum ranges from pure content to pure context.

If you find it difficult to imagine a book on "creativity and innovation in engineering" which is totally concerned with context and never shows or discusses a design at all, you can overcome your difficulty by buying one, for this is the title used by S. A. Gregory's book. It is a collection of papers prepared by the Design and Innovation Group of the University of Aston, Birmingham, under the joint sponsorship of the Working Party on Creativity of the Council Engineering Institution and of the Design Research Society.

Compressed into it are eighteen papers dealing with various aspects of the subject. Inevitably, perhaps, there is a good deal of overlap, but, surprisingly, there are noticeable gaps. For example, we are told on page 81 that "University education in Britain was until lately for the social elite and did not concern itself with the instruction and development of practical engineers at all". To many, like myself, who have been designing for many years by utilizing the realistic principles that we learnt in the Engineering Department at Cambridge, it is a little surprising to learn that, since 1783, the University has solely concentrated on training impractical engineers. Actually students at Cambridge have been required for many years not only to innovate new designs but each of them has his design costed, built and tested, and an account of this and the lectures on creativity would have added much needed substance to the section on education.

Although this book contains a useful summary of theories about the context of design, many of the chapters seem to finish before they have finished. Concluding is common, but conclusions are much rarer. Perhaps the best part of the book is the emphasis on the necessity of industry to exploit innovation and encourage an atmosphere that will breed it, but its general effect is to give the impression that it is easier to break the subject up into little bits than to put it together again to present a unified and persuasive picture.

G. L. GLEGG

CORRESPONDENCE

TAC

SIR,—I read with very great interest your article on the TAC Report (*Nature*, **241**, 2; 1973).

Our company, which through the Greenwich local television channel can claim, we believe, some responsibility for broadening the minds of the legislators, was similarly disappointed with the TAC Report. However, our trade association — the Cable Television Association — has already reacted by stating that it will shortly be producing its own plan for the future of broadcasting in this country. I am convinced that this will promote lively discussion about the great potential and versatility which cable offers.

Yours faithfully,

TIMOTHY DUDMAN

Albion Cablevision Limited,
58 Beauchamp Place,
London SW3 1NZ

Special Relativity Again

SIR,—Professor Ziman's admirable review¹ of Professor Dingle's book *Science at the Crossroads* covers most adequately "the question" raised by Dingle about special relativity, except for one point: Ziman invokes general relativity at a stage when it is not really needed.

In fact, while special relativity does not deal adequately with gravity, it does quite adequately cope with accelerated motion. In special relativity, just as in general relativity, the answer to Professor Dingle's "question" is: the fastest working clock between any two events is one that travels between them by free fall. Any other clock travelling between these events necessarily experiences inertial forces, which a physicist moving with the clock might interpret as being due to a (uniform) gravitational field; a physicist moving with the "fastest" clock would experience no such forces (he would be an "inertial observer").

This completely answers Professor Dingle's "question". It leaves unsettled the further question as to what it is that prescribes this particular structure for space-time. In special relativity, this structure is simply taken as given *a priori*; while this may not be thought to be a completely satisfactory answer (and general relativity gives a better

one), it is certainly at least a logically consistent answer.

Yours faithfully,

G. F. R. ELLIS

Department of Applied Mathematics
and Theoretical Physics,
Silver Street,
Cambridge CB3 9EW

¹ *Nature*, **241**, 143 (1973).

Reprint Requests

SIR,—The interesting article by Briggs and Briggs¹ on reprint request patterns under the deliberately misleading title "Hormones and Blood Chemistry" has moved me to make several comments about the reprint courtesy.

In 1970, some similar experiments on information retrieval techniques were conducted in *Nature*²⁻⁴. At that time, I had a pleasant exchange of correspondence with one of the principals (V. R. Pickles of Cardiff) through which we found ourselves to be in general agreement regarding uses and abuses of the reprint privilege. However, several mistaken impressions still appear to be fairly generally persistent.

There can be no argument that there is abuse of the reprint privilege by people who could determine whether or not they really need a reprint before they order it, by dabblers, and by habitual collectors ("scientific pack rats"). However, there is another side to the requesting of reprints through information gained from sources such as *Current Contents*. Many scientists work in places which are relatively remote from adequate library sources and they must gamble a bit on judgments about *Current Contents* titles to stay in the literature in their fields. If I might use myself as an example, I freely admit to errors in reprint ordering in the past and I acknowledge that I have some reprints which I can't use, but in most of these cases I was misled by titles. It would be impossible for me to check the actual contents of some journals without travelling literally hundreds of miles. I, and others in similar positions, must beg the indulgence of colleagues and we ask not to be lumped with the careless and abusive users of the reprint privilege. Of course, it could be argued that almost everyone should have access to *Nature*, but even this might not be the case.

I also sense that some of the concerns about reprint ordering by American scientists may arise from some mistaken impressions which are held by some workers in other countries concerning general working conditions in science in the United States. While a few workers here may have such financial and/or technical resources available that they can depend exclusively upon computer-based information retrieval done by hired assistants, most of us carry on the day-by-day slog through the literature which is the common burden of scientists everywhere. Hopefully, workable modern information retrieval will continue to become more readily available to everyone everywhere in the next few years.

Finally, one might ask how often these "experiments" need to be conducted in *Nature*. Publication costs and space limitations certainly would enter into such a determination. If further research is needed, possibly the editors could design even better "experiments" if they would construct an occasional *Nature* entry which incorporated into a single title such terms as "cancer, heart disease, racial differences in IQ, energy crisis, ecocide in Vietnam, and biological basis for female superiority".

Yours faithfully,

LELAND G. JOHNSON

Department of Biology,
Augustana College,
Sioux Falls, South Dakota 57102

¹ Briggs, M. H., and Briggs, M., *Nature*, **240**, 490 (1972).

² Davies, D., McKenzie, D. P., Turner, J. S., and Pickles, V. R., *Nature*, **225**, 636 (1970).

³ Pickles, V. R., Davies, D., McKenzie, D. P., and Turner, J. S., *Nature*, **226**, 881 (1970).

⁴ Pickles, V. R., *Nature*, **226**, 1181 (1970).

Nature's Parish

SIR,—At this juncture to ask the "academic community" to give Dr Kissinger "credit for his liberalizing influence in the past four years" is remarkable. As the use of violence during the Kissinger era has been liberal to the utmost extent, this statement (*Nature*, **241**, 1; 1973) ought, in fact, to be remembered and highly valued. The editor should be given credit for having given the ultimate expression to the complicity of the aca-

demic community in the crucifixion of Vietnam.

Yours faithfully,

KNUT ROGNES

Zoologisk Laboratorium,
Universitetet I Bergen

Who was HeLa?

SIR,—It is twenty-one years since George Gey established the famous HeLa cells in culture. It has been estimated that the weight of these cells in the world today exceeds that of the American negro from whose cervical tumour they originated. That lady has achieved true immortality, both in the test-tube and in the hearts and minds of scientists the world over, since the value of HeLa cells in research, diagnosis, etc., is inestimable. Yet we do not know her name! It has been widely stated that He and La are the first letters of her names but whereas one textbook says the names were Helen Lane another says Henrietta Lacks. My letters to the authors, inquiring the source of their information, like the letter to the hospital from which Gey's paper emanated, remain unanswered. Does anyone know for sure? Would it be contrary to medical ethics in the HeLa cell's coming-of-age year to authenticate the name and let He . . . La . . . enjoy the fame she so richly deserves?

Yours faithfully,

J. DOUGLAS

Department of Applied Biology,
Brunel University

Entropy and Vitalism

SIR,—Without even having read my book¹, Van Kley² refers to it as "a new form of vitalism" such that for evolution "different forms of the laws of thermodynamics apply". This is such a gross misinterpretation that I am compelled to object.

On page 22 I state: "I think our classical notions of entropy as they come to us from the presently established laws of physics and chemistry are totally inadequate in dealing with the living system. This does not mean that there is anything mysterious, supernatural, or vitalistic about the living system. It simply means that our classical notions are inadequate".

I should like to stress the word inadequate. For example, the laws of Newtonian mechanics are totally inadequate in explaining the shift in the perihelion of Mercury. Einstein's equations, which explained this quantitatively, are different in the sense that they are more general; Newton's equations are just a special case.

The concept of entropy in informa-

tion theory is far more general than in classical thermodynamics. Specifically, the entropy, H , as defined by Shannon³, is

$$H = -K \sum_i p_i \log p_i \quad (1)$$

where the p_i are probabilities of elementary events on a finite probability space and K is an arbitrary constant. If the p_i are all equal, then

$$H = -K \log p_i \quad (2)$$

or

$$H = k \log W \quad (3)$$

where W is the total number of elementary events on the space. But (3) is Boltzmann's definition of the thermodynamic entropy which appears as a special case under Shannon's more general definition.

Schrödinger⁴ foresaw that we have given a positive name, entropy, to a negative concept—a measure of a kind of disorder. He proposed that we use the negative value of the entropy, the "negentropy", as a measure of the order or organization. I believe that Schrödinger was wrong. The true measure of the organization is the maximum value of the entropy, H^{Max} , minus the value we actually observe, H^{Obs} . H^{Obs} as a measure of the disorder has no structure, but $H^{Max} - H^{Obs}$ as a measure of the organization is rich in mathematical structure which classical theory neglected but which my theory stresses. It is in this sense a re-definition and extension of the entropy concept.

Consequently, I believe my work reduces the aura of vitalism man has always associated with the living system.

Finally, Van Kley certainly cited the wrong reference for any anti-evolutionary statement. Chapter 9 of my book is initiated by the following quotation from "The Giants" by Kahlil Gibran.

"I am among those who believe in the Law of Evolution; I believe that ideal entities evolve, like brute beings, and that religions and governments are raised to higher planes.

"The Law of Evolution has a severe and oppressive countenance and those of limited or fearful mind dread it; but its principles are just, and those who study them become enlightened."

Yours faithfully,

LILA L. GATLIN

Space Sciences Laboratory,
University of California, Berkeley

¹ Gatlin, L. L., *Information Theory and the Living System* (Columbia University Press, New York, 1972).

² Van Kley, H., *Nature*, **240**, 365 (1972).

³ Shannon, C. E., *The Mathematical Theory of Communication* (University of Illinois Press, Urbana, 1949).

⁴ Schrödinger, E., *What is Life?* (Cambridge University Press, London, 1944).

Synthetic Food

SIR,—The present is an especially opportune time for the initiation of a massive, interdisciplinary programme of research and development on the total synthesis of food.

Political as well as scientific leaders are coming to realize that agriculture, in the race with population, can at best only maintain the present 2,000-calorie-a-day diet in the developing countries. The "Green Revolution" and other recent advances are serving to gain time, but in a few years the population will outstrip the food supply unless the growth of population is quickly checked—an unlikely possibility—or unless an independent source of food is developed—a possibility that can be realized.

Two circumstances favour the immediate initiation of a major programme for the total synthesis of food. First, there is the availability of many scientists, engineers, and other experts who are now unemployed and would respond with alacrity to a new and challenging opportunity. Second, industry is at a stage at which it could adapt the vast fund of scientific knowledge and engineering experience amassed in the manufacture of synthetic polymers to the production of food.

Why has not a start been made? The answer lies in the problem of securing support for a programme of sufficient magnitude and duration to assure success. Experience in the administration of research has shown that support for a major, imaginative new programme can be obtained only after those proposing the programme have already made a significant beginning on their own resources. Research laboratories today that are competent to undertake a programme on the total synthesis of food already have a full complement of productive projects. Thus a new programme could be undertaken only at the sacrifice of currently successful activities.

The situation is similar to that which led to the beginning of the plantation rubber industry in 1876. Henry Wickham, later Sir Henry, discovered the unusually quick germinating characteristics of the seed of the *Hevea brasiliensis*. He chartered a steamer to bring seedlings growing in baskets of earth from the Amazon to London. Sir William Hooker, Director of Kew Gardens, threw out a collection of rare orchids to make space for the tender, little known seedlings until they should be ready to send to Ceylon, and later to Malaya. Since that time the billions of rubber trees on plantations have all been descendants of these original specimens.

Are there Britons today who have the

vision and the courage of Wickham and Hooker of a century ago?

Yours faithfully,

ARCHIBALD T. MCPHERSON

4005 Cleveland Street,
Kensington, Maryland 20795

Announcements

University News

Professor Paul Harrison Temple, University of Dar es Salaam, has been appointed to the **Chair of Geography** at the University of Birmingham, from June 1973.

Miss M. Hulme, Withington Girls' School, Manchester, has been appointed to the **University Grants Committee**.

Mr E. D. Mason, Director of Education, County Borough of Tees-side, has been appointed to the **University Grants Committee**.

Professor L. P. Harvey, Professor of Spanish at Queen Mary College, has been appointed to the **Cervantes Chair of Spanish** at King's College, London.

Miscellaneous

Professor R. T. Williams, St Mary's Hospital Medical School, has been awarded the CIBA Medal and Prize of the Biochemical Society for 1972.

Dr John M. Ashworth, University of Leicester, has been awarded the Colworth Medal of the Biochemical Society for 1972.

Professor D. J. Crisp, University College of North Wales, has been appointed to a visiting Professorship at the Federal University of Ceara at Fortaleza and will make subsidiary visits to Rio de Janeiro, São Paulo, Recife, and spend a short period in Argentina and Chile.

Dr R. Lee Clark, president of the University of Texas, M. D. Anderson Hospital and Tumor Institute, at Houston, has been chosen the recipient of the **Sidney Farber Medical Research Award**, previously given to Mary Lasker, Sir Alexander Haddow and Senator Lister Hill.

Dr Kent R. Van Horn, Vice-President of the Aluminium Company of America, has been awarded the **Institute of Metals (Platinum) Medal**.

Professor D. Hull, Department of Metallurgy and Materials Science at the University of Liverpool, has been awarded the **Rosenhain Medal** for his contribution in the field of physical metallurgy.

Erratum

In the article "Normal Incidence Radiation Trends on Mauna Loa, Hawaii" by R. F. Pueschel *et al.* (*Nature*, **240**, 545; 1972) the second transmission equation

$$\begin{aligned} \text{in Table 1 should read } T = & 0.9075 + \\ & 0.000005175t - 0.00272 \sin \left(\frac{2\pi t}{365} \right) \\ & + 0.00164 \cos \left(\frac{2\pi t}{365} \right) \end{aligned}$$

Addendum

In the article "Mercury in Lake Sediments; a Possible Indicator of Technological Growth" by Aston *et al.* (*Nature*, **241**, 450; 1973), the term parts per billion (p.p.b.) was not defined. The usage throughout was American; 1 p.p.b. is equivalent to one part in 10^9 .

International Meetings

March 12, **Electron Sources for Microscopy and Related Techniques**. (Meetings Officer, Institute of Physics, 47 Belgrave Square, London SW1.)

March 12-15, **American Society for Neurochemistry**. (Centre for Continuing Medical Education, The Ohio State University College of Medicine, 320 West Tenth Avenue, Columbus, Ohio 43210.)

March 12-16, **Symposium on Nuclear Data in Science and Technology**. (Dr L. Hjärne, Nuclear Data Section, Division of Research and Laboratories, International Atomic Energy Agency, PO Box 590, Vienna A-1011, Austria.)

March 14, **Aspects of Process Development**. (Assistant Secretary, Society of Chemical Industry, 14 Belgrave Square, London SW1.)

March 14-15, **Sensory Appraisal of Difficult Foods**. (Dr D. N. Rhodes, Meat Research Institute, Langford, Bristol.)

March 15, **Spectroscopy and Anemometry by Photon Correlation Methods**. (The Institute of Physics, 47 Belgrave Square, London SW1.)

March 19, **Pesticides and the Processed Food Industries**. (Conference Secretary, Society for Chemical Industry, 14 Belgrave Square, London SW1.)

March 20, **Soil Responses to Long Periods Under Uniform Management**. (Executive Secretary, 14 Belgrave Square, London SW1.)

March 25-30, **International Symposium on Hepatotoxicity**. ("KENES", Organizers of Congress and Special Events Ltd, PO Box 16271, Tel Aviv, Israel.)

March 26-27, **Spring Conference for High Speed Photography**. (C. W. Husbands, Central Unit for Scientific Photography, Royal Aircraft Establishment, Farnborough, Hampshire.)

March 26-28, **Conserving Our Resources**. (Conference Secretary, Society for Chemical Industry, 14 Belgrave Square, London SW1.)

Reports and Publications

not included in the Monthly Books Supplement

Great Britain and Ireland

- Statistics of Smoking in the United Kingdom. Edited by G. F. Todd. (Research Paper 1, 6th Edition.) Pp. 132. (London: Tobacco Research Council, 1972.) [3011]
- A Key to the Freshwater Fishes of the British Isles, with Notes on Their Distribution and Ecology. By Dr. Peter S. Maitland. (Scientific Publication No. 27.) Pp. 139. (Far Sawrey, Ambleside: Freshwater Biological Association, 1972.) £1.20. [112]
- Wildfowl 23. Pp. 144+16 plates. (Slimbridge: The Wildfowl Trust, 1972.) £1.75; \$5.50. [112]
- The Royal Society. Report of Council for the year ended August 31, 1972. Pp. 98. (London: The Royal Society, 1972.) [112]
- The British Council. Annual Report 1971/1972. Pp. 95. (London: The British Council, 1972.) [112]
- Estuarine Research: a Report on the Natural Environment Research Council Estuaries Forum held in the Summer of 1971. Pp. 20. (Publications, Series C, No. 8.) (London: Natural Environment Research Council, 1972.) [112]
- Bulletin of the British Museum (Natural History). Zoology, Vol. 24, No. 3: Miscellanea. Pp. 157-228. (London: British Museum (Natural History), 1972.) £3.40. [412]
- Life with Diabetes. By Arnold Bloom. Pp. 30. (London: British Medical Association, 1972.) 13p. [412]
- University of Oxford. Annual Reports, 1970/1971. Pp. 27. (Oxford: The University, 1972.) 50p. [412]
- Proceedings of the University of Newcastle upon Tyne Philosophical Society. Vol. 2, No. 1: The German Linear Atlas. By A. W. Stanforth. Pp. 1-14. (Newcastle upon Tyne: University of Newcastle upon Tyne Philosophical Society, 1972.) [512]
- Covent Garden Community Association. Report. Pp. 25. (London: Covent Garden Community Association, 1 Shelton Street, 1972.) [512]
- Technical and Specialised Periodicals Published in Britain: a Selected List. Pp. 264. (London: Central Office of Information, 1972.) [612]
- Some Fundamental Aspects of Urea Technology. By Dr S. M. Lemkowitz, Dr M. G. R. T. de Cooker, and Professor P. J. van den Berg. Pp. 102. (London: The Fertiliser Society, 1972.) [612]
- Philosophical Transactions of the Royal Society of London. B: Biological Sciences. Vol. 264, No. 864: The Growth of Children at Different Altitudes in Ethiopia. By E. J. Clegg, I. G. Pawson, E. H. Ashton and R. M. Flinn. Pp. 403-437. (London: The Royal Society, 1972.) 90p; £2.50. [612]
- Fifth Report of the Countryside Commission for the year ended 30 September, 1972. Pp. vii+52. (London: HMSO, 1972.) 57p net. [712]
- Department of the Environment. Getting the Best Roads for Our Money: The COBA Method of Appraisal. Pp. 17. (London: HMSO, 1972.) 32p net. [712]
- Loch Morar Survey. 1972 Report. Pp. 8. (London: Loch Morar Survey, 80 Palewell Park, SW14, 1972.) [712]
- A Little Book About Our Hearing, How We Measure Sound, How We Can Protect Ourselves Against Noise and What We Can Do To Make Noisy Machines Quieter. Pp. 14. (Hemel Hempstead, Herts: Atlas Copco, 1972.) [812]
- Education: A Framework for Expansion. (Cmnd. 5174.) Pp. iv+49. (London: HMSO, 1972.) [812]
- PEP Broadsheet No. 539: Overseas Nurses in Britain: A PEP Survey for the United Kingdom Council for Overseas Student Affairs. By Michael Thomas and Jean Morton Williams. Pp. iii+54. (London: Political and Economic Planning, 1972.) £1. [812]
- Father of the Man. By Professor T. D. Foster. (Inaugural Lecture delivered in the University of Birmingham on 9 March, 1972.) Pp. 17. (Birmingham: The University, 1972.) 25p. [1212]
- Into Action: Plan for a Modern Employment Service. Pp. 27. (London: The Employment Service, Department of Employment, 1972.) [1212]
- Hillingdon Natural History Society. Bird Report, 1965-1970. Pp. 55. 50p. Mammal and Reptile Report, No. 1. By A. R. J. Paine. Pp. 9. 30p. Mammal and Reptile Report, No. 3. Edited by A. R. J. Paine. Pp. 16. 35p. (Uxbridge, Mddx.: Hillingdon Natural History Society, 4 Heron Close, 1970 and 1971.) [1212]
- Department of the Environment. Design Bulletin No. 26. New Housing and Road Traffic Noise—a Design Guide for Architects. Pp. 29. (London: HMSO, 1972.) 25p. [1312]
- Government and High Technology. By John Jewkes. (Third Wincott Memorial Lecture delivered at the London School of Economics and Political Science, 31 October, 1972.) Pp. 24. (London: The Institute of Economic Affairs, 1972. Published for the Wincott Foundation.) 50p. [1312]
- Patterns of Research. Pp. 44. (Newcastle-upon-Tyne: Procter and Gamble, Ltd., 1972.) [1312]
- Tin Chemicals for Industry. (TRI Publication No. 447.) Pp. 32. (Perivale, Greenford, Middx.: Tin Research Institute, Fraser Road, 1972.) [1412]
- Industrial Relations Training. Pp. 14. (London: The Commission on Industrial Relations, 140 Gower Street, WC1, 1972.) *Gratis*. [1512]
- Zoology Leaflet No. 3: The Giant Panda, *Ailuropus melanoleuca* (David). Pp. 4. (London: British Museum (Natural History), 1972.) 3p. [1512]
- United Kingdom Atomic Energy Authority: Research Group. Report AERE-R 7245: Radioactive

Fallout in Air and Rain—Results to the middle of 1972. By R. S. Cambray, Miss E. M. R. Fisher, D. H. Peirson and A. Parker. Pp. 47. (Harwell, Berkshire: Health Physics and Medical Division, AERE, 1972. Available from HMSO.) £1 net. [1512]
 Proceedings of the Royal Irish Academy. Vol. 72, Section A, No. 10: Kinematics Angular Momentum and Eulerian Dynamics in Hilbert Space. By J. L. Synge and P. Yodanis. Pp. 121-148. 52p. Vol. 72, Section B, No. 17: Anticancer Agents—VII. Derivatives of Pyruvaldehyde and Ethylpyruvaldehyde. By Joan Byrne and J. F. O'Sullivan. Pp. 295-306. 20p. Vol. 72, Section B, No. 18: The Stratigraphy and Structure of the Lower Palaeozoic Rocks of Eastern Murrisk, Co. Mayo. By J. McManus. Pp. 307-334+plate 12. 40p. Vol. 72, Section B, No. 19: The Rockabill Granite, Co. Dublin. By J. C. Brandley and P. S. Kennan. Pp. 335-346+plate 13. 24p. (Dublin: Royal Irish Academy, 1972.) [1512]

Other Countries

Canada: Ministry of State. Scientific Activities: Federal Government Costs and Expenditures 1963-64 to 1972-73. Pp. 40. (Ottawa: Information Canada, 1972.) [1412]

National Museums of Canada. Ontario Prehistory: an Eleven-Thousand-Year Archaeological Outline. By J. V. Wright. Pp. 120 (28 plates). Archaeological Survey of Canada. Paper No. 1: An Archaeological Survey Between Cape Parry and Cambridge Bay, N.W.T., Canada, in 1963. By William E. Taylor, Jr. Pp. vi+106 (15 plates). Paper No. 2: The Aberdeen Site, Keewatin District. By J. V. Wright. Pp. 97. Canadian Centre for Folk Culture Studies. Paper No. 1: Vampires, Dwarves, and Witches Among the Ontario Kashubs. By Jan L. Perkowski. Pp. 85. Ethnology Division. Paper No. 1: Preliminary Study of Traditional Kutchin Clothing in Museums. By Judy Thompson. Pp. 92. Paper No. 2: Sarcophagi Paragons. By Eung-Do Cook. Pp. 50. Paper No. 3: Gambling Music of the Coast Salish Indians. By Wendy Bross Stuart. Pp. 114. (Ottawa: National Museums of Canada, 1972.) [1412]

European Organization for Nuclear Research—CERN. Proceedings of the 1972 CERN School of Physics (organized in collaboration with ICTP, Trieste), Grado, Italy, 15-31 May, 1972. Pp. ix+597. (Geneva: CERN, 1972.) [1412]

World Directory of Venereal Disease Treatment Centres at Ports/Repertoire Mondial des Centres de Traitement pour Maladies Vénériennes dans les Ports. Third edition/Troisième édition. Pp. 196. (Geneva: World Health Organization; London: HMSO, 1972.) 24 Sw. francs; £2.40; \$6. [1512]

Smithsonian Contributions to Zoology. No. 110: Review of the Genus *Cerceris* Latreille in Mexico and Central America (Hymenoptera: Sphecidae). By Herman A. Scullen. Pp. 121. (Washington, DC: Smithsonian Institution Press, 1972. For sale by US Government Printing Office.) \$1.50. [1512]

Fisheries Research Board of Canada. Technical Reports. No. 341: An Actograph for Laterally or Ventrally Compressed Aquatic Organisms of Medium Size. By D. J. Wildish and S. M. Polar. Pp. 14. No. 344: Determination, Toxicity, and Environmental Levels of Phthalate Plasticizers. By V. Zitko. Pp. 35. (St. Andrews, N.B.: Fisheries Research Board of Canada, Biological Station, 1972.) [1512]

Science Council of Canada. Special Study No. 24: Air Quality—Local, Regional and Global Aspects. (Background Study for the Science Council of Canada.) Pp. 39. (Ottawa: Information Canada, 1972.) 75 cents. [1512]

National Research Council of Canada. Annual Report on Support of University Research/Compte Rendu Annuel sur l'Aide Apportée à la Recherche

Scientifique dans les Universités, 1971-1972. Pp. 618. (Ottawa: Science Research Council of Canada, 1972.) \$2.50. [1512]

Bulletin of the Museum of Comparative Zoology, Harvard University. Vol. 144, No. 3: Social Biology of the Neotropical Wasp *Mischocyttarus drewseni*. By Robert L. Jeanne. Pp. 63-150. Vol. 144, No. 4: Studies in the Milliped Order Chordeumida (Diplopoda): a Revision of the Family Cleidionidae and a Reclassification of the Order Chordeumida in the New World. By William A. Shear. Pp. 151-352. (Cambridge, Mass.: Museum of Comparative Zoology, Harvard University, 1972.) [1612]

The Environment Film Review: a Critical Guide to Ecology Films. Pp. 155. (New York: Environment Information Center, 124 East 39th Street, 1972.) \$20. [1612]

Smithsonian Contributions to Zoology. No. 134: Additions to Revisions of the Biennial Fish Genera *Esenius* and *Enomacrodus*, with Descriptions of Three New Species of *Esenius*. By Victor G. Springer. Pp. 13. (Washington, DC: Smithsonian Institution Press, 1972. For sale by US Government Printing Office.) 30 cents. [1612]

United States Department of the Interior: Geological Survey. Water-Supply Paper 1798-K: Fluvial Sediment in Salem Fork Watershed, West Virginia. By Russell F. Flint. Pp. iv+29. 30 cents. Professional Paper 599-J: The Contributions of Ranger Photographs to Understanding the Geology of the Moon. By N. J. Trask. Pp. iii+16. 40 cents. Professional Paper 737: Recreation in Open-Channel Flow. By J. P. Bennett and R. E. Rathbun. Pp. ix+75. \$1. Professional Paper 753: Geophysical, Geohydrological, and Geochemical Reconnaissance of the Luke Salt Body, Central Arizona. By Gordon P. Eaton, Donald L. Peterson and Herbert H. Schumann. Pp. iii+28. 40 cents. (Washington, DC: Government Printing Office, 1972.) [1612]

EUROSTAT S.A.: Aims, Prospects, Organisation, Financing. Pp. 30. (Geneva: EUROSTAT, 17, Rue Pierre Fatio, 1972.) [1712]

Carnegie Institution of Washington. Report of the President, 1971/1972. Pp. 45. (Washington, DC: Carnegie Institution of Washington, 1972.) [1212]

Philosophy of Medicine and Science: Problems and Perspectives. Compiled by Department of Philosophy of Medicine and Science. Pp. 287. (New Delhi: Institute of History of Medicine and Medical Research, 1972.) [1212]

US Department of the Interior: Geological Survey. Water-Supply Paper 1798—1: Fluvial Sediment in Hocking River Subwatershed 1 (North Branch Hunters Run), Ohio. By Russell F. Flint. Pp. iv+23. 30 cents. Professional Paper 716-A: Geology and Copper Mineralization of the Saindak Quadrangle, Chagai District, West Pakistan. By W. Ahmed, S. N. Khan and Robert G. Schmidt. Pp. v+21+1 plate. (Washington, DC: Government Printing Office, 1972.) [1212]

Australia: Commonwealth Scientific and Industrial Research Organization. Annual Report of the Division of Tropical Pastures. 1971/1972. Pp. 113. (Brisbane, Qd.: CSIRO, 1972.) [1212]

International Atomic Energy Agency. Technical Reports Series No. 137: Mercury Contamination in Man and his Environment. Pp. 181. (Vienna: IAEA; London: HMSO, 1972.) 140 Austrian schillings; £2.30; \$6. [1312]

Office de la Recherche Scientifique et Technique Outre-Mer. Mémoires ORSTOM. No. 56: Etude de Sols Formes sur Roches Carbonatées-Pédogenèse Fersiallitique au Liban. Par Maurice Lamouroux. Pp. 266. No. 57: Sols sur Socle Ancien à Madagascar—Types de Différenciation et Interprétation Chronologique au Cours du Quaternaire. By Fernand Bourgeat. Pp. 335. (Paris: ORSTOM, 1972.) [1312]

The Meaning of Race. By Phillip V. Tobias. Second edition—revised and enlarged. Pp. 41. (Johannesburg: South African Institute of Race Relations, 1972.) 60c. [1412]

Groups That Can Help: A Directory of Environmental Organizations. Pp. 12. (Washington, DC: US Environmental Protection Agency, 1972. For sale by the US Government Printing Office.) 15 cents. [1512]

Bulletin of the American Museum of Natural History. Vol. 149, Article 1: Systematics and Behavior of South American Flickers (Aves, *Colaptes*). By Lester L. Short. Pp. 1-100. (New York: The American Museum of Natural History, 1972.) \$4. [1512]

Australia: Commonwealth Scientific and Industrial Research Organization. Division of Soils Technical Paper No. 14: Comparison of Current Chemical Methods for Evaluating Irrigation Soils. By J. Loveday, H. J. Beatty, and (the late) J. M. Norris. Pp. 19. (Melbourne: CSIRO, 1972.) [1812]

World Meteorological Organization. Reports on Marine Science Affairs. Report No. 4: Requirements for Marine Meteorological Services. (WMO No. 288.) Pp. 22. (Geneva: WHO, 1971.) [1812]

International Labour Office, Geneva. Occupational Safety and Health Series. No. 27: Safety and Health in Shipbuilding and Ship Repairing. Pp. iii+236. (Geneva: International Labour Office, 1972.) [1812]

Environmental Education in the Community College. By Arden L. Pratt. Pp. vii+112. (Washington, DC: American Association of Community and Junior Colleges, 1 Dupont Circle, 1972.) [1812]

Organization for Economic Co-operation and Development, Paris. The Engineering Industries in OECD Member Countries—New Basic Statistics, 1963-1970. Vol. 1: Shipments of "100" Selected Products. Pp. 100. (Paris: OECD; London: HMSO, 1972.) 11 francs; 90p; \$2.50. [1812]

Institut Royal Météorologique de Belgique. Publications, Série A, No. 77: Climatologie Aérologique. Station d'Uccle (06447), Institut Royal Météorologique de Belgique, coordonnées: Latitude 50° 48' N, Longitude 04° 48' E, Altitude 100 m. Par P. Defrise, R. Sneyers, W. Struylaert, and J. Van Isacker. Pp. 42. (Uccle-Bruxelles: Institut Royal Météorologique de Belgique, 1972.) [1812]

Australia: Commonwealth Scientific and Industrial Research Organization. Annual Report of the Wheat Research Unit, 1971/1972. Pp. 26. (Rydge, NSW: CSIRO, 1972.) [1812]

Australian Journal of Zoology, Supplementary Series, No. 14: A Revision of the Psednirini (Orthoptera: Pyrgomorphidae). By K. H. L. Key. Pp. 72. (East Melbourne: Editorial and Publications Section, CSIRO, 372 Albert Street, 1972.) [1812]

Geophysical Surveys, Vol. 1, No. 1, September 1972. Edited by Wm. Markowitz. Pp. 1-120. Subscription price per volume of 4 issues (approx. 500 pages). Dfl. 170; \$55.25, including postage. Private persons (no institutions, laboratories, libraries, etc.) may subscribe at the reduced rate of Dfl. 70; \$22.75 per volume, including postage. (Dordrecht: D. Reidel Publishing Company, P.O. Box 17, 1972.) [1912]

Health Hazards of the Human Environment. Prepared by 100 Specialists in 15 Countries. Pp. 387. (Geneva: World Health Organization; London: HMSO, 1972.) 44 Sw. francs; £4.40; \$11. [1912]

Australia: Commonwealth Scientific and Industrial Research Organization. Annual Report of CSIRO Minerals Research Laboratories, 1971/1972. Pp. 52. (Melbourne: CSIRO, 1972.) [1912]

Science in the Federal Republic of Germany: Organization and Promotion. Second revised and enlarged edition. By Reinhold Geimer and Hildegard Geimer. Pp. 76. (Bonn—Bad Godesberg: DAAD, Deutscher Akademischer Austauschdienst, 1972.) [1912]

Editorial, Advertising and Publishing Offices of NATURE

MACMILLAN JOURNALS LIMITED
 4 LITTLE ESSEX STREET, LONDON WC2R 3LF
 Telephone Number: 01-836 6633. Telegrams: Phusis London WC2R 3LF
 Telex 262024

MACMILLAN JOURNALS LIMITED
 711 NATIONAL PRESS BUILDING
 WASHINGTON DC 20004
 Telephone Number: 202-737 2355. Telex 64280

International Advertisement Manager
 PETER R. KAVANAGH
 MACMILLAN JOURNALS LIMITED
 4 LITTLE ESSEX STREET, LONDON WC2R 3LF
 Telephone Numbers: UK 01-836 6633. USA 202-737 2355

Subscription Department
 MACMILLAN JOURNALS LIMITED
 BRUNEL ROAD, BASINGSTOKE, HANTS RG21 2XS
 Telephone Number: Basingstoke 29242

Classified advertisements
 T. G. SCOTT & SON, LIMITED
 1 CLEMENT'S INN, LONDON WC2A 2ED
 Telephone Number: 01-242 6264/01-405 4743
 Telegrams: Textualist London WC2A 2ED

Registered as a newspaper at the Post Office

Copyright © Macmillan Journals Limited, March 9 1973

HOW TO BUY NATURE

The cost of one year's subscription to NATURE is:

	UK & elsewhere	US & Canada
Nature (Friday)	£16	\$48
Nature & Nature Physical Science or Nature New Biology	£24	\$83
Nature New Biology or Nature Physical Science	£10	\$35
All three editions	£29.50	\$108

(Charges for delivery by air mail on application). Subscribers in North America may be able to claim a tax rebate against their NATURE subscription.